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AN ANALYSIS OF PERCEPTUAL STYLES: ARE THERE REDUCERS AND AUGMENTERS?

by

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Duke University

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Dissertation submitted in partial fulfillment of
the requirements for the degree of Doctor
of Philosophy in the Department of
Psychology in the Graduate School
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ABSTRACT

(Psychology-Clinical)

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Individuals may experience the same event differently. This paper explores one factor among many that could contribute to individual differences in perception: a continuum of amplification-attenuation of sensory experience. Different operationalizations of this concept were analyzed.

Petrie (1967) developed the concept of a reducing-augmenting continuum and contended that "reducers" subdue their sensory experience while "augmenters" amplify it. She used a kinesthetic size judgment task with intervening stimulation with large and small blocks between sets of judgments. Her measure of augmentation-reduction was change scores calculated by subtracting post-stimulation scores from baseline (pre-stimulation) scores. She contended that, regardless of the size of block used for intervening stimulation, individuals consistently increased/decreased their size judgments. The kinesthetic task with intervening stimulation has usually been used to induce kinesthetic contrast effects,

and change scores have been viewed as reflecting those contrast effects. Although Petrie's idea was new, her measure remained grounded in the contrast effects task. Subsequent studies associating the kinesthetic task with perception in other modalities blurred the distinction between augmentation-reduction and contrast effects.

Experiment 1 in this study was an attempt to distinguish between contrast effects and augmentation-reduction. The data suggested that Petrie's change score measure contained no factor other than contrast effects. An alternative measure of an amplifying-reducing style was suggested: baseline kinesthetic size judgments with no intervening stimulation. It was found that baseline judgments were reasonably reliable and that individuals differed in their size judgment. Size judgments ranged both above and below the actual size of the stimulus, suggesting that some individuals amplify and others attenuate kinesthetic size.

Experiment 2 examined three perceptual style hypotheses: Petrie's augmentation-reduction factor measured by kinesthetic change scores; a contrast effects interpretation of change scores; and an amplification-attenuation hypothesis with baseline kinesthetic size judgments as its basic measure. Subjects were also given a pain tolerance task and loudness and kinesthetic magnitude estimation tasks to see if their performance on the kinesthetic measures was related to their performance on tasks in other modalities.

Performance patterns within the kinesthetic tasks provided

additional evidence against Petrie's augmentation-reduction interpretation of kinesthetic change scores. There was slight support for a weak consistency in kinesthetic contrast effects which accounts for only a small part of the variance of change scores. The consistency of baseline kinesthetic size judgments was confirmed.

None of the kinesthetic measures was related to pain or to the magnitude estimation tasks, nor were these latter tasks related to each other. Thus, there was no evidence for a generalized perceptual style based on any of the hypotheses and measures tested.

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E. R. B.

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CHAPTER I

INTRODUCTION AND LITERATURE REVIEW

That individuals process the same events and stimuli differently has been a theme in both the arts and in psychology. Unravelling an individual's interpretation and experience of an event is often the focus of work in therapy. This study will explore one dimension among many that might contribute to individual differences in perception and experience: the continuum of amplification-attenuation of sensory experience.

The concept of an amplification-attenuation continuum originated with Petrie (Petrie, 1960, 1967; Petrie & Collins, 1961; Petrie, Collins, & Solomon, 1960), who described individual differences in modulation of intensity of sensory experience. She divided the continuum of modulation into three perceptual types: the reducer, who decreases what is perceived; the augmenter, who increases what is perceived; and the moderate, who neither reduces nor augments. This reduction-augmentation construct was invoked in subsequent research in the modulation of intensity of sensory experience.

Petrie's (1967) reducer-augmenter construct bears some similarity to earlier work in perceptual styles which centered around two conceptual

frameworks: perceptual defense (Eriksen, 1952, 1965; Eriksen & Browne, 1956; Lazarus, Eriksen, & Fonda, 1951) and cognitive styles (Gardner, Holzman, Klein, Linton, & Spence, 1959; Holzman & Gardner, 1959). The perceptual defense framework developed the concept of perceptual styles in response to threat or conflict, while the cognitive style framework extended this notion into conflict-free areas as well. However, even in the cognitive style literature, the idea that a psychological defense, usually repression, is generating a particular cognitive style is never abandoned; there is not a full commitment to the concept of a primary perceptual style. The present paper carries the notion of conflict-free perceptual styles to its logical conclusion in the attempt to study a primary perceptual style of the amplification or attenuation of sensory experience.

It is interesting to note how pervasive an influence the notion of repression has been in perceptual style research. The perceptual style code words in the perceptual defense and cognitive style literature are defense-vigilance (Eriksen, 1952, 1965; Eriksen & Browne, 1956; Eriksen et al., 1951), repressers-sensitizers (Byrne, 1961, 1964), levellers-sharpeners (Gardner et al., 1959; Holzman & Gardner, 1959). The concept of repression is explicitly invoked in some of this literature. The research in reduction-augmentation has tended to steer clear of referring itself to repression, but the gross parallel with the earlier perceptual style concepts is obvious.

The research in augmentation-reduction suggests some interesting intraindividual consistencies. However, reviewing this research raises questions as to how to understand the evidence that has accumulated in support of the augmentation-reduction concept. The experiments reported in this paper were designed to clarify the nature of these consistencies and to see if they are maintained through increasing conceptual and operational clarity.

Individual Differences in Modulation of Sensory Experience

In 1967 Asenath Petrie published Individuality in Pain and Suffering in which she argued for consistent individual differences in perception of both neutral and painful stimuli. This book was the culmination of work that developed and was summarized in earlier articles (Petrie, 1960; Petrie & Collins, 1961; Petrie, Collins, & Solomon, 1958, 1960). Petrie argued that people differ in their modulation of the intensity of sensory experience, in their "perceptual reactance." As has been seen, she divided the continuum of modulation into three perceptual types: the reducer, the augmenter, and the moderate. They process their experience of the sensory environment differently, the reducer decreasing what is perceived, the augmenter increasing what is perceived, and the moderate neither reducing nor augmenting what is perceived.

Petrie used changes in judgments of kinesthetic size as her basic measure of augmentation-reduction. The task requires the subject, while

blindfolded, to judge the width of a block (the standard) held between the thumb and forefinger of the dominant hand. The subject's size judgment consists of his indicating with the non-dominant hand the spot on a tapered block that feels just as wide as the standard. Between sets of four judgments, subjects rub another block with thumb and forefinger of the dominant hand. The total test involves two sets of tasks as described above administered at least 48 hours apart. In one administration the intervening rubbing block is larger, and in the other smaller, than the standard.

Such kinesthetic tasks have traditionally been used to measure contrast aftereffects (Bakan & Thompson, 1967; Bakan & Weiler, 1963; Broadbent, 1961; Carlson, 1963; Eysenck, 1955; Gibson, 1933; Hilgard, Morgan, & Prytulak, 1968; Koehler & Dinnerstein, 1947; Morgan & Hilgard, 1972; Nachmias, 1953; Platt, Holzman, & Larson, 1971; Spitz & Lipman, 1960; Wertheimer, 1955; Wertheimer & Herring, 1968; Wertheimer & Leventhal, 1958) and conceptually comprise an extension of work on visual figural aftereffects (Gibson, 1933; Köhler &Wallach, 1944). These tasks usually involve stimulation with blocks larger and/or smaller than the standard. Rubbing a larger block results in a tendency to make a decreased judgment of the size of the standard and rubbing a smaller block results in a tendency to increase the size judgment. Petrie (1967) argued that, although contrast effects do operate, there is a tendency for some people to increase and others to decrease size judgments,

regardless of the relative size of the rubbing block. Her criterion for a reducer was someone who decreases size more than 5.4 millimeters on one test (usually the large block test, though Petrie does not mention this fact) and who decreases or only slightly increases size judgments on the other test (usually the small block test). The augmenter increases his size judgment more than 5.4 mm on one test and increases or only slightly decreases on the other test. Moderates are defined as those who change less than 5.4 mm on both tests. Petrie (1967) reported significant correlations of +.60 (for a group of 13 female college students) and +.77 (for a group of 28 adults) between change scores on the large and small block tests, such that the augmentation-reduction construct can be said to account for about half the variance on the kinesthetic task even when contrast effects are operating in opposite directions. Furthermore, she argued that a perceptual style consisting chiefly of strong susceptibility to contrast effects, i.e., large increases with small block stimulation along with large decreases with large block stimulation, is quite atypical. She called this style "stimulus-governed" which she found chiefly among brain-damaged adults and rarely in normal adults. In two groups totaling 106 juvenile delinquents, Petrie (1967) found 20% stimulus governed in each group, while only 3 of 92 (3%) control subjects were reported as stimulus governed. Morgan, Lezard, Prytulak, and Hilgard (1970) used Petrie's criteria and found five stimulus-governed subjects among 42 (12%), proportionally higher than the frequency Petrie (1967) reported.

Petrie (1967) interpreted a strong susceptibility to contrast effects as a lack of the stabilizing influence of a consistent perceptual modulation style (reducer, augmenter, or moderate). This lack results in a relatively unstable perceptual world and leaves the individual prone to the influences of intervening stimulation. Comparing stimulus-governed delinquents with augmenting, reducing, or moderate delinquents, Petrie found almost half of the stimulus-governed delinquents had been charged with five or more criminal acts, while this was true for only one fifth of the other delinquents. Furthermore, in notes by supervisors who were in constant contact with them, the stimulus governed were more often described as "immature, unpredictable, changeable" (41% of the stimulus governed versus 10% of the other delinquents). Petrie hypothesized that the stimulus-governed style may reflect either an abnormality or a developmental immaturity. Testing the developmental hypothesis, Petrie (1967) compared groups of adults, 8-12 year olds, and 6-7 year olds. She found that the younger the group, the greater the difference between large and small block test scores. Petrie also hypothesized that subjects would show greater susceptibility to contrast effects in their non-dominant hand because it is less frequently used and has developed less control. She found that the difference between small block and large block scores was significantly greater for the non-dominant hand. Thus Petrie placed the kinesthetic task as a measure of contrast effects in a very different perspective from earlier work. She contended that the kinesthetic task

should be viewed chiefly as a measure of a consistent perceptual style, augmentation-reduction, rather than of--or in addition to--contrast effects.

The Augmentation-Reduction versus Contrast Effects Problem

In spite of the cluster of data lending support to the augmentation-reduction construct, Petrie's (1967) argument basically rests on the positive correlations she found between change scores on the large and small block tasks and on the atypicality of the stimulus-governed style. She then used the augmentation-reduction construct to generate hypotheses about people's behavior in other tasks and situations. At times her hypotheses seem to be actual predictions, at other times merely explanations of observed events. Petrie's hypotheses were usually confirmed, and this confirmation and her explanations became her extension of and further evidence for the augmentation-reduction construct. However, although the construct is new and the predictions generated from it are new, Petrie never totally freed her measure from the kinesthetic aftereffects framework. It will become apparent that the distinction between augmentation-reduction and contrast effects is repeatedly blurred in Petrie's and subsequent studies.

Before continuing to review Petrie's hypotheses, it would be well to note how her measures remained grounded in the aftereffect framework, since this is important in evaluating the meaning of her evidence.

In the tasks she used, kinesthetic size judgments before and after large and small block intervening stimulation, the intervening stimulation induces contrast aftereffects. Her measures were change scores--post-stimulation minus baseline scores, which is exactly how aftereffects have been measured. The only difference is the interpretation Petrie makes of this measure, an interpretation justified by the positive correlation between change scores from large and small block intervening stimulation. Change scores lose information as to the size of a person's judgment relative to the actual size of the object, information that should be important for a perceptual style which is defined as "increasing . . . [or] decreasing what is perceived" (Petrie, 1967). Furthermore, it is unclear why intervening stimulation is necessary to induce augmentation-reduction, particularly why contrast-inducing large and small block stimulation is necessary. Thus, the conceptual distinction between after-effects and the augmentation-reduction continuum is not extended into a direct operationalization in the kinesthetic tasks and measures. The correlations Petrie found between change scores on the large and small block tasks serve as an estimate of the variance that can be attributed to the augmentation-reduction construct, but the measure itself is not a pure measure of augmentation-reduction since aftereffects are clearly involved.

Kinesthetic Tasks and Pain Tolerance

Petrie maintains that reduction and augmentation are not independent aspects of kinesthetic perception, but, rather, generalized perceptual tendencies which are related to input and behavior in other modalities. Her strongest evidence comes from the consistent association between performance on the kinesthetic task--usually change scores on the large block task--and tolerance for pain. On the large block stimulation task, the greater the decrease in size judgments the more tolerant the subject of pain from heat (Petrie et al., 1958) and from the pressure of small projections on the skin (Poser, Note 1; Ryan, & Foster, 1967). In these studies tolerance was measured by amount of heat or pressure the subject was actually willing to endure. Sweeney (1966) found a relationship between kinesthetic change scores and tolerance for pain of the hand held in cold air (0° F). He used two sets of parallel movable flat surfaces, one of which the subject could adjust to indicate his judgment and one of which served as the standard, and when widened, as the stimulation bar. During the stimulation periods of 90 and then 180 seconds, the subject grasped the surfaces, using minimal pressure. Pain tolerance was measured by a 7-point rating scale, with subjects rating their pain every minute for the first 10 minutes of exposure. Subjects who reported little pain on the scale had large decreases on the kinesthetic task, while those who scaled high on pain had small decreases on the kinesthetic task. When the standard itself was used for intervening

stimulation, subjects high and low in tolerance of electric shock did not differ in their kinesthetic change scores (Dinnerstein, Lowenthal, Marion, & Olivo, 1962). Blitz, Dinnerstein, and Lowenthal (1966) used a set of 20 size judgments on a tapered bar with no intervening stimulation between judgments. They found that subjects with less tolerance for electric shock made larger size judgments and subjects with more tolerance made smaller size judgments. This difference was consistently present in the group of subjects whose judgments were made starting from the wider end of the variable, but not in the group in which judgments were made from the narrow end of the variable. Morgan et al. (1970) found no difference between reducers' and augmenters' reports of pain. In the Morgan et al. study, pain was induced by having the subject keep his hand in ice water for 40 seconds. Pain tolerance was measured by subject ratings on a 10-point scale every 5 seconds during exposure to the pain stimulus. Both the large and small block kinesthetic tasks were used to distinguish people as augmenters, reducers, moderates, and stimulus governed, according to Petrie's (1967) criteria.

Several points should be noted about these experimental pain studies. When only large block stimulation is used, Petrie's reducers can be distinguished, but moderates and augmenters must remain indistinguishable except in the rare case in which an augmenter might increase his judgment more than 5.4 mm after large block stimulation. These studies demonstrate an association between pain--both endured and reported--

and change scores on the kinesthetic task. With only large block stimulation, however, it is unclear whether the change scores are reflecting Petrie's augmentation-reduction factor or contrast effects. Dinnerstein et al. (1962) used the standard itself for intervening stimulation, a method which does not induce contrast. They found no association between pain tolerance and kinesthetic change scores, which implies that contrast effects may account for the association between pain tolerance and large block change scores in other studies. Morgan et al. (1970) was the only study in which both large and small block stimulation was used. They found no association between pain tolerance and classification as augmenter or reducer. However, in the Morgan et al. study duration of pain stimulation was quite short and tolerance was measured by report only. In the Blitz et al. (1966) study there was no intervening stimulation and therefore no change scores. Size judgments (in descending series but not in ascending series) were related to the amount of electric shock endured--the larger the size judgment the lower the quit point. Thus the Blitz et al. study gives partial support to the view that absolute size judgment rather than aftereffect is associated with pain tolerance.

Petrie (1967) also presented data of tolerance for pain in a clinical setting. Among eight mothers whose delivery was normal, the augmenters bore the pain of childbirth least easily. Petrie did not state which of the kinesthetic tasks was used to determine augmentation-reduction (large or small or both). Pain tolerance was measured by a physician's and

nurse's ratings based on patients' demands for drugs, sleeplessness due to pain, physical signs, and patient statements. The physician and nurse were not aware of the patients' kinesthetic scores. Petrie (1967) also cited evidence from individual clinical cases. A man who experienced no pain from a peptic ulcer showed an unusually large decrease score after large block stimulation--around 14 mm below baseline as compared with average reducers' 6-9 mm decrease. A man with severe pain from a phantom limb showed an unusually large increase score after stimulation with the small block--around 13 mm as compared with an augmenting group's average of about 6 mm. Combined means of change scores on large block and small block tasks for a photophobic boy reached an 11 mm increase by the end of the stimulation periods. Only in this last case is it clear that both large and small block stimulation were used.

Some more generalized hypotheses about the relationship between augmentation-reduction and pain tolerance were confirmed. Petrie reasoned that, since pain serves a protective function, those who are less sensitive to it (reducers) would be less preoccupied with signs of ill health and less careful in avoiding painful injuries. She cited a study by Solon showing significant negative correlation between the Hypochondriasis scale on the MMPI and degree of reduction, a study by Schonfield showing greater concern about health among children who were augmenters, and a much higher percentage of accidents for reducers coming to health services than for augmenters in Solon's study. Among tattooed

delinquents, tattooing being a painful process, 63% were reducers and 0% augmenters (Petrie, 1967). Details of determination of augmentation-reduction are not given for these studies.

Petrie regards pain as a particular case of overstimulation, and her prediction that reducers are the most tolerant of such stimulation is supported by most of the available data. On the other end of the stimulation spectrum, she predicted that reducers will be less tolerant of sensory deprivation than augmenters because the limited stimulation available to them is perceived as less intense. In a sensory deprivation experiment, subjects with large decreases in kinesthetic size judgments after stimulation stayed in an iron lung apparatus a shorter amount of time than those with increases or small decreases on the kinesthetic task. Petrie did not state whether one or both block tests were used, but the data suggest that only the large block task was used. In an institution for juvenile delinquents, every reducer expressed preference for pain over solitary confinement (Petrie, 1967). Non-reducers' preferences were not reported. In the expectant mother study, the reducers were rated by hospital staff as behaving worst during restriction in the months prior to delivery (Petrie, 1967). Again, details of determination of augmentation-reduction were not given.

Drug and Audioanalgesia Studies

In another set of experiments, Petrie (1967) explored the effects of different drugs on augmentation-reduction. She found that both alcohol

and aspirin significantly increased reduction in kinesthetic size judgments following large block stimulation. Breakdown of the subjects into augmenters, reducers, and moderates indicated that the drug effect for the whole group was almost wholly the contribution of the augmenters: Augmenters no longer augmented and even reduced to some degree when on alcohol or aspirin, while there was little change in moderates or reducers--reducers even reduced a little less. Only large block stimulation was used, so that Petrie's usual criteria for categorizing subjects, particularly augmenters, could not be used. It is therefore unclear how subjects were designated augmenters, reducers, and moderates. Judging from her data, it appears that augmenters may have been those who increased, moderates those who decreased slightly, and reducers those who decreased by more than 5.4 mm on the large block task. Petrie suggested that the pain-reducing effects of alcohol and aspirin could be due to their ability to increase the tendency to reduce--especially for the augmenter. In the alcohol study, Petrie also found alcohol to increase endurance of physical discomfort and fatigue, as measured by the length of time a subject could hold his leg in an unsupported position. Morgan et al. (1970) found that under hypnosis augmenters reduced their pain more than reducers, even though the two groups did not differ on hypnotizability scores on the Stanford Hypnotic Susceptibility Scale. In the Morgan et al. study augmenters and reducers were classified according to Petrie's criteria by scores on both large and small block tasks.

Although the Morgan et al. (1970) study supports the notion that augmenters are more responsive to methods of pain reduction, Petrie's alcohol study itself provides data to undercut this interpretation, though Petrie does not view it as such. In the alcohol study, Petrie (1967) included a control session so that each subject had grapefruit juice once with and once without vodka in it. There was also a pretesting session in which subjects' degree of augmentation-reduction was determined by using the large block intervening stimulation task. In both the alcohol and "feigned alcohol" sessions, augmenters as classified on the pretesting session "reduced." Petrie (1967) interpreted this as an important placebo effect, almost as strong as the effect of alcohol itself. However, the results could also be interpreted as regression towards the mean: Since it is unusual to find increases with large block intervening stimulation, those who increased ("augmented") on the first trial would be likely to decrease on subsequent trials with no experimental manipulation at all. The regression effect also explains the fact that the reducers reduced a little less: Since "reducers" are also extreme scorers, on retest they would be expected to score less extremely, i.e., "reduce" less. Petrie recognizes the possibility of statistical regression towards the mean, but views it as only "part" of what is going on. However, she has no way of estimating to what extent to ascribe her results to statistical artifacts.

Petrie's (1967) work with audioanalgesia presents a similar picture. Considering audioanalgesia a pain-reducing procedure, Petrie predicted

that subjects doing the kinesthetic task while hearing white noise of moderate intensity through earphones (the typical audioanalgesic procedure) would "reduce" more, i.e., decrease more: Signed change scores would shift downward, such that increases would become smaller or become decreases and decreases would decrease further. Again, only the large block task was used, and it is clear from Petrie's data tables that she classified as "augmenters" those who showed increases after large block intervening stimulation in the no audioanalgesia condition. Since increases after large block stimulation have a relatively low probability of occurring, the regression of extreme scores would result in increasers ("augmenters") not repeating the increase, but rather moving towards the group mean, which for them would involve a shift downward, i.e., an apparent reduction. Petrie's (1967) results in fact fit a regression effect explanation: "Augmenters'" (increasers') scores indeed shifted downward, while "moderates" and "reducers" (those who initially showed decreases, the more common response to large block intervening stimulation) showed little change. Although Petrie (1967) interpreted this as the differential effect of pain-reducing methods on augmenters as opposed to reducers and moderates, regression effects provides a simpler, more parsimonious, and probably more accurate explanation.

Petrie (1967) hypothesized that alcohol would be most attractive to those on whom it had the greatest effect, and predicted that alcoholics would tend to be augmenters. Of 12 alcoholic patients, half were

augmenters and half were moderates; none were reducers. Here Petrie used both large and small block tasks to distinguish augmenters, reducers, and moderates. This group of alcoholics showed significantly less reduction on the large block task than two different "normal" groups.

Most of the pain studies indicate a relationship between performance on a kinesthetic task and pain tolerance in both experimental and clinical situations. The kinesthetic measure is usually change in size judgment after large block stimulation, i.e., post-stimulation measure minus baseline. In spite of Petrie's evidence supporting her construct of augmentation-reduction, the use of large block stimulation in most of the pain studies leaves open the question of whether it is Petrie's augmentation-reduction factor or contrast aftereffect that is related to pain tolerance. In her studies of pain-reducing factors (alcohol, aspirin, audioanalgesia), statistical artifacts could account for the results.

Kinesthetic Tasks and Sensory Experience in Other Modalities

A few studies explored the relationship between performance on kinesthetic size tasks and sensory experience in other modalities. Petrie (1967) found a significant correlation of +.53 between change scores for kinesthetic width judgments (averaging change scores for both large and small block stimulation) and change scores for judgments of weight in a task in which subjects judged weight before and after holding a lighter and heavier weight (change scores for the heavier and lighter conditions were

averaged). Sales and Throop (1972) found a low but significant positive correlation between amount of decrease in judgment after Petrie's large block stimulation task and slopes of auditory magnitude estimation functions. Cavonius, Hilz, and Chapman (1974) found a positive correlation between percent of change in kinesthetic size judgment following Koehler and Dinnerstein's (1947) basic kinesthetic task and brightness magnitude estimation exponents. The kinesthetic task in the Cavonius et al. study induced an <u>increased</u> size judgment, while in Sales and Throop's the task induced a <u>decreased</u> judgment. Both found positive correlations between degree of change and magnitude estimation exponents, which suggests it might be degree of contrast aftereffect that is associated with sensitivity in other modalities. However, two aspects of the Cavonius et al. (1974) study qualify this conclusion. Cavonius et al.'s change score is a percent change score, reflecting both baseline and change scores, and it is unclear how the two varied together and how much each contributed to the correlation. Furthermore, the instructions Cavonius et al. gave their subjects in the magnitude estimation experiment differed from the usual magnitude estimation instructions in not mentioning proportionality when they told subjects to increase numbers with increased brightness. It is unclear how this difference affected results, though it does seem that the derived exponents were somewhat lower than those in the usual brightness magnitude estimation experiments. Thus, the set of data from these studies leaves open the question of whether it is contrast effects or

augmentation-reduction or possibly baseline judgment that is associated with perception in other modalities. Within each study, no conceptual distinction is made between augmentation-reduction and aftereffects--the two are used more or less interchangeably.

Research in neurophysiology supports the notion of individual differences in responsiveness to intensity of sensory stimulation. This research involves average evoked cortical responses (AER) to photic and auditory stimuli. Subjects have responded differently to increasing intensities of stimulation. For some, evoked responses increase in amplitude with increasing stimulus intensity and for others evoked responses do not increase, or even decrease, with increasing stimulus intensity (Blacker, Jones, Stone, & Pfefferbaum, 1968; Buchsbaum, 1971, 1975, Note 2; Buchsbaum & Pfefferbaum, 1971; Buchsbaum & Silverman, 1968; Schechter & Buchsbaum, 1973; Silverman, Buchsbaum, & Henkin, 1969; Soskis & Shagass, 1974; Spilker & Callaway, 1969). These different AER patterns are compatible with the interpretation that some people amplify and, particularly, that some people attenuate, their sensory experience. AER amplitude-intensity functions are related to performance on a kinesthetic task similar to Koehler and Dinnerstein's (1947) basic model. Correlation coefficients between linear slopes of AER amplitude-intensity functions and change scores on the kinesthetic task range between +.60 and +.78, such that people who increase size judgments or barely decrease them have steeper AER amplitude-intensity

slopes, and those with greater decreases in size judgments have flatter AER amplitude-intensity slopes. This relationship holds both when the kinesthetic task is inducing increases in size judgments (Spilker & Callaway, 1969) and when it is inducing decreases in size judgments (Blacker et al., 1968; Buchsbaum & Silverman, 1968; Silverman et al., 1969). These studies taken together indicate that it is not degree of contrast effects that is associated with AER, but rather the factor Petrie (1967) labelled augmentation-reduction. It should be noted that Spilker and Callaway (1969) used sine wave modulated light to elicit evoked responses, while the other studies used flashes of light. These two methods are not equivalent (Soskis & Shagass, 1974), so that kinesthetic tasks inducing increases and those inducing decreases have been associated with AERs evoked by different methods.

Buchsbaum and Silverman (1968) and Buchsbaum (Note 2) have reinterpreted the concept of augmentation-reduction. They postulate that reducers are actually more sensitive at lower intensities and that reduction is a protective adjustment for stimulation at higher intensities. This differs from Petrie's (1967) view which sees augmenters as amplifying and reducers as attenuating stimuli at any level of intensity. Buchsbaum's reinterpretation is interesting in that it moves reduction a step closer to the concept of repression, by making reduction a defensive mechanism of sorts. In other articles, Buchsbaum and his co-workers have postulated an attentional factor involved in augmentation-reduction (Schechter

& Buchsbaum, 1973; Williams, Bittker, Buchsbaum, & Wynne, 1975). Numerous articles discuss possible relationships between augmentation-reduction and schizophrenia and affective disorders (Borge, 1973; Buchsbaum, 1975, Note 3; Buchsbaum, Goodwin, Murphy, & Borge, 1971; Buchsbaum, Landau, Murphy, & Goodwin, 1973; Landau, Buchsbaum, Carpenter, Strauss, & Sachs, 1975; Silverman, 1964, 1967, 1972). Both kinesthetic tasks and AERs have been used in studies of brain damage and "dysfunction" (Buchsbaum & Wender, 1973; Klein & Krech, 1952; Petrie, 1967). In short, there is a proliferation of theorizing and research generated by the augmentation-reduction concept, but much of it ignores some basic questions as to what is really involved in this concept. Almost every study cited blurs the distinction between contrast aftereffects and augmentation-reduction--either conceptually or operationally, and often both.

Clusters of studies on the subject of augmentation-reduction are consistent and suggestive in their results, the critique of some of Petrie's (1967) studies notwithstanding. Do these clusters actually converge to support the hypothesis that there are consistent individual differences in modulation of sensory experience, such that some individuals amplify and some attenuate their sensory experience?

Reliability of Kinesthetic Measures

I have already noted difficulties in previous studies in separating augmentation-reduction and contrast effects. Another difficulty in

answering the question about consistent individual differences arises because of research casting doubt on the between-session reliability of change scores on kinesthetic tasks. Between-session reliability is necessary if a measure is to be interpreted as a consistent perceptual style of an individual. Petrie (1967) reported split-half reliabilities of +.97 for the sum of the first and fourth change scores in each set of four measurements over both large and small block tasks and the sum of the second and third change scores in each of these sets of measurements. This procedure sums scores across days so that it does not provide a measure of between-session reliability. The correlations of +.60 and +.77 between change scores on the large and small block tests that Petrie (1967) reported do comprise a between-session measure because the large and small block tasks are administered at least 48 hours apart. Some other studies confirm within-session reliability but not between-session reliability. For the large block task change scores, Spitz and Lipman (1960) found within-session test-retest correlations of +.74 with a 18-20 minute rest period between tests and of +.34 with only 5 minutes rest between tests. The group with 20 minutes rest also got feedback before their second test, and it is unclear how this feedback affected reliability. The group with only 5 minutes rest did not get feedback. Platt et al. (1971) reported within-session reliabilities ranging from +.56 and +.70 for change scores for a small block kinesthetic task. The Platt et al. study and, to some extent, the Spitz and Lipman study (1960)

support the within-session test-retest reliability of change scores. Between-session repeated measures of change scores on a large block task yielded nonsignificant correlations of +.18 (Morgan & Hilgard, 1972) and of +.15 to +.33 for a small block task (Platt et al., 1971). When Morgan and Hilgard (1972) corrected for the residual aftereffect of Day 1 on Day 2 baseline by calculating change scores on both days from the initial Day 1 baseline, large block change score test-retest reliability between sessions shifted upward to +.59. Two studies failed to replicate a significant positive correlation between large and small block change scores. Broadhurst and Millard (1969) found a nonsignificant correlation of +.27 between change scores on large and small block tasks in a group of 20 females. However, they did not determine the time in menstrual cycle of their subjects; Petrie (1967) has suggested that in the premenstrual phase women exhibit more stimulus-governed tendency than at other times. Morgan et al. (1970) reported a low but significant negative rank order correlation of -.34 between large and small block stimulation change scores, which indicates the salience of contrast effects. When the correction for residual aftereffects used by Morgan and Hilgard (1972) was applied to the data in the Morgan et al. (1970) study, the large block-small block correlation became a positive +.33. Thus, the findings as to the test-retest reliability of kinesthetic change scores and correlations between large and small block change scores are equivocal. This is further complicated--or perhaps explained--by psychometric difficulties

involved in change scores, and particularly in the reliability of change scores (Bereiter, 1963; Lord, 1963; McNemar, 1969; Morgan & Hilgard, 1972). Calculating correlations for change scores is problematic because the equations involved have psychometric properties which tend to push results into specific ranges. Reliability of change scores depends on the reliabilities of pre- and post-measures and on their correlation with each other. Correlations between change scores and independent variables will differ depending on which of a number of possible correlational formulas one chooses. When pre- and post-measures are highly correlated with each other and are each highly reliable (as is the case with kinesthetic task measures), change scores will tend to be unreliable. Change scores also raise issues of regression effects, of compounding the error in pre- and post-measures, and of the loss of information contained in pre- and post-scores by leaving only their difference. Choice of a meaningful way to correct for some of the above problems is also problematic: There is an array of correctional procedures each having its own limitations and biases. Morgan and Hilgard's (1972) effort to correct for possibly biased baselines on the second day of testing is an example of the difficulty in choosing and understanding the meaning of the possible correctional procedures. Their "corrected" correlations were quite different from their "uncorrected" correlations, and it is not clear how to interpret nor how certain to be about each one.

In summary, there is evidence to support the contention that

performance on the kinesthetic task is related to other aspects of perception. It remains unclear, however, whether one should conceive of this association as reflecting contrast effects or a reduction-augmentation factor. Furthermore, the between-session reliability of change scores--crucial to an argument for individual, consistent styles--is questionable. Experiment 1 was designed to analyze aspects of the kinesthetic task relevant to these issues.

CHAPTER II

THE FIRST STUDY

Experiment 1 was an attempt to differentiate between contrast effects and an augmentation-reduction factor. Furthermore, an alternative measure for augmentation-reduction was considered.

The conceptual blurring of augmentation-reduction and contrast effects is perpetuated by the use of large and/or small block intervening stimulation. Contrast effects could be minimized by substituting a stimulation block the same size as the standard. In this way, an augmentation-reduction factor could be operationally isolated and its association with Petrie's (1967) measure tested. If Petrie's kinesthetic tasks measure something other than contrast effects, there should be a positive correlation between change scores (post-stimulation minus baseline measures) regardless of the size of the block used for intervening stimulation. This follows from Petrie's argument that even large and small block change scores are positively correlated. It is to be expected that a similar task which does not induce contrast would be positively correlated with Petrie's tasks--even more highly correlated with each of Petrie's measures than her two measures are with each other.

An alternative measure of "augmentation-reduction" was considered for several reasons. First, change scores are statistically problematic. In addition to reliability problems, change scores lose information contained in pre- and post-scores. Such information seems particularly important for a perceptual style of amplifying or attenuating. For example, should a person whose baseline and post-stimulation judgments are much larger than another's be viewed in the same way as a person with the same pre-post change but whose initial and final scores are both smaller? Furthermore, it was unclear why intervening stimulation should be necessary to induce augmentation-reduction rather than its being immediately reflected in individuals' size judgments. For this reason, "perceptual modulation" was operationalized as the difference between an individual's size judgments and the actual size of the object. With a standard of constant size, then, an individual's baseline or post-stimulation judgments could serve as his amplifying-attenuating score. In this way, change scores could be avoided and information as to the subject's actual size judgment would be retained. Furthermore, the correlation between baseline and post-stimulation measures could be ascertained to see if intervening stimulation changed an individual's score relative to others.

It was reasonable to expect that baseline and post-stimulation measures would be reliable. Correlations between subjects' baseline measures on different days have ranged from +.51 to +.77 (Morgan et al.,

1970; Morgan & Hilgard, 1972; Platt et al., 1971). With large block intervening stimulation, Morgan and Hilgard (1972) found between-session correlations of +.74, +.71, and +.79 for first, second, and third post-stimulation measures, respectively. Unlike Morgan and Hilgard, Platt et al. (1971) did not consider phase in menstrual cycle of their female subjects. However, correlations for between-session post-stimulation scores with small block intervening stimulation for males were +.85 and +.87 for first and second post-stimulation scores, respectively. (The parallel correlations for females were +.14 and +.50.)

Although this alternative operationalization was logical, even if it satisfied reliability criteria two other questions required consideration. One was whether size judgments varied above and below actual size in a way that might be reflecting "augmentation" or "reduction" of the stimulus. The other was whether this alternative operationalization was related to Petrie's augmentation-reduction measure.

Experiment 1 introduced a same size block task to minimize contrast effects. This task was the same as Petrie's (1967) kinesthetic task except that it involved intervening stimulation with a block the same size as the standard rather than with a larger or smaller block. However, this is not Petrie's measure, and it was necessary to test whether an individual's performance on the same block task is related to Petrie's task. Since the majority of studies relating the kinesthetic task to pain tolerance used large block intervening stimulation, Petrie's large block

task was administered in Experiment 1 to see how it related to the same block intervening stimulation task.

Petrie suggested a 45-minute hand rest period before the subject begins the block task. She said this is necessary in order to avoid the unpredictable effect of augmentation-reduction induced by previous activity but gives no indication of how 45 minutes was determined as the necessary amount of rest time. Morgan and Hilgard (1972) used a 45-minute rest period while Platt et al. (1971) used an unspecified shorter rest period. Platt et al. nevertheless found between-session correlations of the same magnitude as Morgan and Hilgard for all their measures--baseline, post-stimulation, and change scores. Thus it seemed that the longer rest period did not enhance the stability of any of the measures. It was therefore decided to use a 15-minute rest in Experiment 1. A smaller group of subjects was given the tasks preceded by a 5-minute rest period to see if the rest period could be reduced to that extent.

In summary, Experiment 1 attempted to answer the following questions:

1. Is there an augmentation-reduction factor in Petrie's (1967) kinesthetic change scores that cannot be attributed to contrast effects? If so,

Can this factor be operationally isolated from contrast effects?

2. Do people consistently amplify or attenuate their kinesthetic size judgments relative to the actual size of the stimulus? If so,

How do these size judgments relate to Petrie's (1967) measure of augmentation-reduction?

Method

Subjects

Twenty-four male acquaintances of the experimenter who agreed to participate in the study served as subjects. They ranged in age from 20-38. Males were chosen to avoid possible complicating factors of time in menstrual cycle for females. The study was run with 16 subjects having a 15-minute hand rest period before each of the tasks, and with 8 subjects having a 5-minute hand rest period before each of the tasks. Three of the 16 subjects and 1 of the 8 subjects were left handed.

Apparatus

The apparatus used was a duplication of that described by Petrie (1967) for the large block intervening stimulation task, with an additional 1.5-inch block to be used for same block stimulation. The apparatus consisted of four smooth, unpainted wooden blocks. Two blocks were 6 inches long and 1.5 inches (38 mm) wide. One of these served as the standard and the other for intervening stimulation. The "large block" was 6 inches long and 2.5 inches wide and was used for intervening stimulation. The fourth block was 30 inches long and tapered from 0.5 inch at its narrow end to 4 inches at its wide end. The width of this block was marked in 1.5 and 2 mm intervals so that fairly accurate readings could

be made to the 0.5 mm. Subjects indicated their size judgments on this tapered block. Each of the four blocks was equipped with sliding finger guides. Whenever a block was in use, it was placed on a small wooden stand so that the subject's fingers would touch only the block and not the table.

Procedure

The basic design of the study was as follows:

Day 1: hand rest period same block task	Day 2: hand rest period same block task hand rest period large block task

Day 1 and Day 2 were separated by a 48-hour to 1-week interval.

The subject was read instructions suggested by Petrie (1967) at the beginning of the first session.

> We are going to explore your sense of touch in these two fingers [the experimenter demonstrates thumb and index finger] on each hand. There are no right or wrong answers. I am interested in the way in which <u>you</u> experience things; the only right answers are what you feel. Let me explain what we are going to do. We'll place your right hand [if the subject is right handed; the reverse if he is left handed] on a horizontal wooden block of a certain width, then, while you're still holding this block we'll place your left hand on a horizontal block that tapers from a small width at the bottom to a greater width at the top. I will ask you to find a place on the tapered block that feels just as wide as the block in your right hand. Because the sense of touch is influenced by what you have been doing with your hands, you must rest these two fingers [the experimenter demonstrates thumb and index finger] of both hands before we can use the blocks. Nothing should touch them while they rest. You can cross your arms or put them in any position so long as you keep these fingers from touching each other or anything else.

These instructions were followed by a 15- (or 5-) minute hand rest

period. At the end of this time, the experimenter gave similar instructions with a little more detail on Day 1, telling the subject that he would also be rubbing other blocks between sets of judgments. On Day 2 this repetition seemed superfluous and was deleted (see Appendix A for the full text of instructions).

The subject was then blindfolded and seated in front of the apparatus. The subject's dominant hand was placed on a 1.5-inch (38 mm) wide block (the standard), with thumb and index finger between finger guides that could move the length of the block. The subject's nondominant hand was placed at the narrow end of the tapered block with the subject's thumb and index finger between finger guides. The subject was allowed to feel that the right (dominant) hand block was of equal width all along its length and that the left (nondominant) hand block tapered. The subject was told to find "a spot on the tapered block which feels just as wide as the block in [your] other hand." After two practice trials, the subject made four baseline judgments. After these judgments, he rested the hand that was on the tapered block, and rubbed another block (same size, 1.5 inches or large, 2.5 inches) for 90 seconds with his dominant hand. After the rubbing period, his dominant hand was placed back on the original block (the standard) and his other hand back on the tapered block. He again made four judgments on the tapered block, indicating "a spot on the tapered block which feels just as wide as the block in [his] other hand." A

similar process was repeated with two 120-second rubbing periods.[1]

Results and Discussion

The format of the subjects' score sheet is presented in Figure 1. Measurements were in millimeters. Each subject had three such sheets, one for same block task on Day 1 (to be labelled "1"), one for same block task on Day 2 (to be labelled "2"), and one for the large block task on Day 2 (to be labelled "L"). In all three administrations the standard was 1.5 inches (38 mm) wide. In the discussion that follows, "baseline" refers to the mean of the four baseline trials. "Post-stimulation score" refers to the mean of the 12 post-stimulation trials. "Change score" refers to the post-stimulation score minus the baseline. Thus, change scores with a negative sign mean there was a decrease in the size of the judgment after intervening stimulation, and positive change scores mean there was an increase.

The data are presented separately for the subjects who had a 15-minute hand rest period (N = 16) and the subjects who had a 5-minute rest period (N = 8). Most of the discussion that follows refers to the 15-minute rest group unless otherwise indicated.

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[1]Petrie (1967) gave subjects three stimulation periods: 90, 90, and 120 seconds. Due to an oversight, this experimenter administered stimulation periods of 90, 120, and 120 seconds to the 16 subjects in the 15-minute rest group. For the 8 subjects in the 5-minute rest group, the stimulation periods were like Petrie's. This oversight had little bearing on the results, as will be seen in the data on effect of stimulation.

	Practice trials		Trials 1	2	3	4	Average of 4 trials		Difference from baseline average
Baseline							baseline		
After 90-sec. stim.									
After 210- (180) sec. stim.									
After 330- (300) sec. stim.									
							mean post-stimulation score		mean change score

Figure 1. Score sheet.

Of primary importance is whether change scores on the large block and same block tasks are correlated. Because the same size block intervening stimulation task was administered twice, two estimates of the same block-large block change score association are available. The two correlations are -.28 and +.14, neither of which is statistically significant. The fact that they are in opposite directions suggests that this nonsignificance indeed approximates a zero correlation and that there is no common factor in change scores with same block versus large block intervening stimulation. This argues against Petrie's (1967) contention that large block change scores reflect an augmentation-reduction factor rather than contrast effects and suggests that large block change scores reflect chiefly contrast effects. There remains the possibility that Petrie's augmentation-reduction factor is really some type of interaction with contrast effects, but this is not Petrie's argument, and it is difficult to explain how or why such an interaction would operate consistently within individuals.

Experiment 1 was also designed to answer whether people consistently amplify or attenuate their kinesthetic size judgments relative to the actual size of the stimulus.

Individual baseline scores did reflect an amplification-attenuation of actual size across individuals, with mean individual baseline scores ranging from 30 to 51 mm and single baseline judgments ranging from 29 to 53 mm for a 38-mm block. This range of more than 20 mm is

much broader than the average individual range in baseline judgments, which spanned only 2.8 mm.

Average individual standard deviations also give an estimate of intra-individual variability in size judgments. Average individual standard deviations were calculated by taking the mean of individuals' standard deviations in the group of 16 subjects. The average individual standard deviations for the first six baseline measures on Day 1 and Day 2 were 1.9 and 1.8 mm, respectively. The average individual standard deviation for the 12 post-stimulation measures (3 sets of 4 measures) on Day 1 was 2.2 mm. Group standard deviations (across individuals) were 4.1 and 4.8 mm for baseline on Day 1 and Day 2, respectively, and 4.9 and 5.5 mm for post-stimulation score on Day 1 and Day 2, respectively.

Individuals were fairly consistent in their size judgments across sessions. Between - test correlations between the same measures in the different tasks (1, 2, and L) are presented in Table 1. The correlations r_{12} are between-session reliability measures for baseline and the post-stimulation measures on the same block task. All of these are significant for the 15-minute rest group ($p < .05$). The first four rows are based on four measures each; the fifth row is based on 12 measures (the post-stimulation score based on the three sets of post-stimulation measures). The stability of size judgments is also apparent in the correlations between baseline and post-stimulation scores (see Table 2). For the group of 16 subjects who had 15-minute hand rest periods, all of these

Table 1

Between-Test Correlations of the Same Measures

15-minute rest (N = 16)				5-minute rest (N = 8)			
	r_{12}	r_{2L}	r_{1L}		r_{12}	r_{2L}	r_{1L}
Baseline	.62	.86	.48	Baseline	.79	.21	.38
90	.72			90	.34	.52	
210	.75			180	.01	.81	
330	.76			300	.56	.70	
Post-stimulation score	.82	.75	.48	Post-stimulation score	.27	.79	.39

Note. For N = 16, correlations above .48 are significant (p = .05); for N = 8, the correlation must reach 0.7 for p = .05.

Table 2

Correlations Between Baselines and Post-Stimulation Scores

	15-minute rest (N = 16)	5-minute rest (N = 8)
1	$.72^{*}$	.65
2	$.90^{*}$	.55
L	$.65^{*}$	$.84^{*}$

$^{*}\underline{p} < .05$.

correlations are significant and fairly high. Even for the large block task, in which there is a contrast effect, there is a consistency between initial and post-stimulation judgments.

For the 5-minute rest group (N = 8), between-session correlations are low and not significant for any of the post-stimulation measures, although the baseline measure is significantly reliable. It appears that the stability of baseline judgments is less vulnerable to a short rest period than post-stimulation measures, though the small group size qualifies this conclusion.

The results presented here suggest that change scores on the large block task reflect contrast effects since large block change scores share no variance with change scores on the same block intervening stimulation task which does not induce contrast effects. Size judgments themselves seem to reflect a consistent amplification or attenuation of the stimulus. The consistency of individuals' size judgments is fairly robust: It is evident between sessions in all measures on the same block task, in the relationship between baseline and post-stimulation measures with and without contrast-inducing stimulation, and within the Day 2 session in high correlations between same block and large block measures both before and after stimulation. Furthermore, these size judgments vary considerably across individuals, with averages of the four baseline judgments ranging from 30 to 51 mm for a 38-mm block. Thus we can speak of a consistent amplification or attenuation of kinesthetic size judgments

which is reflected in a subject's baseline judgments. This position is clearly at odds with Petrie's (1967) operationalization of the augmentation-reduction factor.

It is difficult to compare the amplification-attenuation measure suggested here--baselines--with Petrie's measure--change scores on the large (and sometimes small) block task. This question is psychometrically problematic because it deals not only with change scores, but also with their relationship to measures from which they are partially derived (baseline measures). This circumstance makes efforts at correction particularly difficult. Correlations between large block change score and baseline and post-stimulation measures are presented in Table 3. None of these correlations is significant except the correlation between large block baseline and change scores. Most of the correlations in Table 3 fall in the psychometrically expected range for change scores (Bereiter, 1963), so that is is difficult to know how much meaning to attach to them and the negative trend they suggest. Petrie (1967) reported that "reducers" tend to make larger baseline judgments than "augmenters." Hilgard et al. (1968) found no significant relationship between baseline and change scores. A qualitative examination of the data will be helpful in comparing Petrie's criteria and the measure suggested here.

Table 4 illustrates the distribution of the 16 subjects classified according to post-stimulation score, baseline and Petrie's large block

Table 3

Correlation of Large Block Change Score with Baseline and Post-Stimulation Scores (N = 16)

		Large block change score
Baseline	1	-.31
	2	-.45
	L	-.49*, corrected for regression produced by measurement errors (McNemar, 1969, pp. 177-178)
	L	-.75*, not corrected
Post-stimulation score	2	-.36
	L	.03

*$p < .05$.

Table 4

Distribution of Augmenters and Reducers According to the Various Criteria (N = 16)

	Post-stimulation score ±3.9 mm (mean of 1 + 2)		Baseline score ±3.8 mm (mean of 1 + 2 + L)		Petrie's criterion ±5.4 mm (Lch)
Augmenter	6	(5)	5	(0)	0
Reducer	3	(2)	2	(0)	6
Moderate	7	(7)	9	(6)	10

Note. Numbers in parentheses indicate subject overlap for the criteria in adjacent columns.

criteria. Parentheses indicate number of subjects for whom there is overlap of categories in adjacent columns. Four millimeters above or below actual size was used as the cutoff point for "augmenter" or "reducer" on the kinesthetic task for the measures proposed in this study (actually, one "reducer" was 3.8 mm and one 3.9 mm below actual size). Four mm was used to approximate the magnitude Petrie used, but because there is no contrast effect to boost scores, it was felt that 5.4 mm was too stringent a criterion. Petrie used 5.4 mm average increase or decrease from baseline as her cutoff point for augmenter or reducer. Determination of cutoff points is somewhat arbitrary, since we are actually speaking of a continuum. As can be seen from Table 4, only six of the subjects would fall into the same category on Petrie's measure and on the measure proposed here, and all of these are moderates. Thus Petrie augmenters and reducers are different from baseline (or post-stimulation) amplifiers and attenuators.

In summary, the results of Experiment 1 indicate that: (a) Change scores on Petrie's large block intervening stimulation task are not significantly correlated with change scores after same block intervening stimulation which does not induce contrast effects. This suggests that Petrie's augmentation-reduction measure reflects contrast effects rather than a consistent augmentation or reduction of a stimulus regardless of type of intervening stimulation. (b) There are consistent individual differences in kinesthetic size judgments such that some people consistently judge a

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block to be wider or narrower than its actual size. (c) There is little subject overlap between amplifiers-attenuators on the basis of kinesthetic size judgments and augmenters-reducers on the basis of Petrie's large block task and criterion.

The consistent individual differences in kinesthetic size judgments support the possibility of a perceptual style varying along an amplifying-attenuating continuum, though of course this style would have to be shown to extend into other modalities before it could legitimately be viewed as an amplifying-attenuating perceptual style. On a general conceptual level, this type of perceptual style is the same as that discussed by Petrie (1967) as augmentation-reduction. However, the analysis and empirical evidence presented here argue that operationally and empirically the amplifying-attenuating style discussed here and Petrie's augmentation-reduction measure are not the same. Numerous studies relate performance on kinesthetic tasks to perception in other sensory modalities and to cortical average evoked responses, but these studies conceptually and operationally blur kinesthetic aftereffects and an amplifying-attenuating factor. Furthermore, psychometric difficulties with change scores make it problematic to tease out an augmentation-reduction factor from aftereffects by purely statistical means without measures independent of the kinesthetic task. Experiment 2 was designed to answer to some of the conceptual and operational issues not addressed in previous studies.

CHAPTER III

THE SECOND STUDY

Experiment 2 was designed to examine the major conceptual and operational elements that have been confounded in previous studies, specifically, contrast effects and an augmentation-reduction factor. The role of augmentation-reduction versus contrast effects in kinesthetic change scores and their association with other variables were examined.

Petrie (1967) argued for an augmentation-reduction factor more powerful than contrast effects. She described this augmentation-reduction factor as involving consistent increases (augmentation), consistent decreases (reduction), or little change after intervening stimulation with both a large or a small block. She supported this contention by finding high positive correlations between change scores after large and small block intervening stimulation. In other words, regardless of the size of the block used for intervening stimulation, individuals were consistent in direction and extent of change, and this consistency overwhelmed any contrast effect.

However, subsequent studies did not replicate the positive correlations Petrie found. Morgan et al. (1970) found a low but significant

negative correlation between change scores after large and small block intervening stimulation. This argues against an augmentation-reduction factor and suggests that there is some consistency in individuals' responses to contrast effects, although this consistency accounts for only a small part of the variance in change scores. Experiment 1, reported in the previous chapter, used intervening stimulation with a block the same size as the standard in order to minimize the influence of contrast effects and test whether change scores based on the same block measure correlated positively with Petrie's (1967) large block measure. A positive correlation between same block and large block intervening stimulation change scores would have supported Petrie's contention that her measures reflect an augmentation-reduction factor rather than contrast effects. However, there was no significant correlation between same block and large block change scores, which suggests that Petrie's large block task reflects chiefly contrast effects. In fact, the only measures that appeared to be consistent regardless of type of stimulation were kinesthetic size judgments as reflected in both baseline and post-stimulation scores.

The studies relating kinesthetic change scores to pain tolerance, to magnitude estimation exponents, and to average evoked cortical responses do not aid in resolving the question of whether it is augmentation-reduction or contrast effects that is associated with these variables because most of these studies use kinesthetic tasks which induce contrast

in only one direction, usually in the direction of decreases in size judgments. In order to resolve the contrast effects versus augmentation-reduction question, subjects' performance on both large and small block tasks must be ascertained and compared with performance on tasks reflecting other variables of interest. Thus, Experiment 2 examined subjects' performance on both large and small block tasks as well as on pain tolerance and magnitude estimation tasks since these latter two measures have been associated with kinesthetic tasks in other studies and viewed as indicative of individual differences in sensitivity.

Kinesthetic baseline measures were also studied in Experiment 2. They were included for several reasons. Unlike change scores, baseline measures have repeatedly been demonstrated to be fairly consistent within individuals across sessions (Morgan & Hilgard, 1972; Morgan et al., 1970; Platt et al., 1971; Experiment 1), a minimum criterion for a perceptual style construct. Furthermore, since baselines are involved in calculating change scores, be it for augmentation-reduction or for contrast effects, it would be useful to have a clearer picture of how baselines are related to other variables. Lastly, the notion that some people might amplify and others attenuate their sensory experience is most simply and directly measured in baseline judgments and their departure from objective size rather than change scores. However, at present there is no empirical evidence to suggest that kinesthetic size judgments are related to other variables reflecting perceptual sensitivity.

Three competing hypotheses were compared: Petrie's augmentation-reduction, contrast effects, and an amplifying-attenuating factor based on baseline measures. Conceptual and operational issues relevant to each hypothesis were addressed on two major levels: verifying basic concepts and measures for each hypothesis and extending the concepts by examining subjects' performance on other perceptual tasks. Minimal requirements for each hypothesis are discussed in the following section, and extension of each concept will be discussed in the subsequent section.

Verifying Basic Concepts and Measures--Minimal Tests

Three sets of concepts and kinesthetic measures have been discussed as descriptive of an hypothesized perceptual style related to perceptual experience in other modalities. Each set must meet certain criteria in order to qualify minimally as a measure of a consistent perceptual style on which individuals differ.

Petrie's Augmentation-Reduction Factor

The lack of relationship between large block change scores and change scores after same size block intervening stimulation found in Experiment 1 suggests that Petrie's (1967) measure of augmentation-reduction is really a measure of contrast effects. Petrie's argument to the contrary rests on the positive correlations she found between change scores on the large and small block tasks (+.60, N = 13; +.77, N = 28). Morgan et al. (1970) did not replicate this finding, in spite of stringent

replication of Petrie's methodology. They found a low but significant negative correlation (-.34, N = 42) between change scores on the large and small block tasks. Experiment 2 will include Petrie's large and small block tasks and test whether signed change scores on both these tasks are positively correlated as Petrie's view would predict.

Contrast Effects

The review of previous research presented above raises the question of whether the associations found in earlier studies between change scores and other variables are accounted for by contrast effects. If there is a style of susceptibility to contrast effects, subjects should demonstrate the same degree of contrast effect whether a task induces increases or decreases in size judgment. Given both large and small block stimulation, the absolute magnitude of the changes on both tasks should be positively correlated and there should be an equivalent negative correlation between signed change.

Notice that the Petrie versus contrast effects hypotheses predict significant large block-small block correlations of opposite sign. If neither hypothesis is true, the correlation between large and small block change scores should approximate zero.

Amplifying-Attenuating Factor

It has been argued here that Petrie's (1967) general concept of a perceptual style on a continuum of amplified-subdued sensory experience

is best measured by kinesthetic size judgments without any intervening stimulation. This is a significant deviation from Petrie's work and, therefore, should not be called "augmentation-reduction." It will be labelled here as "amplification-attenuation" in order to differentiate it from previous work on a similar concept. This amplifying-attenuating factor satisfies the minimal criteria for a consistent perceptual style on which individuals differ. Previous studies (Morgan et al., 1970; Morgan & Hilgard, 1972; Platt et al., 1971) and the results of Experiment 1 indicate that kinesthetic size judgments are reasonably reliable across sessions, with correlations in these studies ranging from +.51 to +.87. The results of Experiment 1 also show a considerable range in size judgments between individuals, from 30 to 51 mm for a 38-mm block. The standard deviation for the group is about 5 mm, while the average individual standard deviation is about 2 mm. Thus, baseline kinesthetic size judgments are a measure on which individuals consistently differ.

Control Factor: Lateral Differences

None of the studies supporting the consistency of kinesthetic size judgments examined the effects of alternating left-right hand placement on the blocks (Morgan et al., 1970; Morgan & Hilgard, 1972; Platt et al., 1971 ; Experiment 1 above). Because the kinesthetic task used in these studies required the subject to make size judgments with one hand as an indication of the perceived size of an object held in the other hand, it is

possible that the size judgment task taps a right-left difference. It is possible that some individuals consistently judge an object in the right hand as larger or smaller than an object of the same size in the left hand, in which case this lateral difference could account for part of the consistency in individuals' size judgments. A lateral difference notion, called the "bilateral kinesthetic difference" by Wertheimer (1954), was briefly studied as a diagnostic indicator for brain damage (McPherson & Renfrew, 1953) and viewed as a source of constant error in kinesthetic aftereffect studies measuring post-stimulation scores without baseline measures (Wertheimer, 1954).

In order to ascertain that individual differences in kinesthetic size judgments reflect perceived size rather than a lateral difference, the relationship between size judgments with left versus right (or dominant versus nondominant) hand was examined in this study by having subjects alternate right-left placement of standard and tapered blocks on different days. If left-right differences are not a major factor in the consistency in size judgments, the correlation between size judgments with dominant versus nondominant hand should be similar to those obtained between sessions when judgments were made with the same hand in both sessions (in the +.51 to +.87 range). This would mean that the kinesthetic size judgments indeed reflect perceived size, and that individuals differ consistently in their size judgments. A negative correlation would indicate that lateral differences are strong and consistent within an individual and

comprise a sufficient explanation for the individual differences in size judgment on the kinesthetic task used in this and other studies, since shifting left-right hand placement would be shifting the previously obtained positive correlation. A zero or nonsignificant correlation would also suggest the presence of lateral differences, but it would not provide as powerful a statement of their strength and consistency.

Up to this point, only minimal criteria for the different perceptual style hypotheses have been discussed. In order for any of the above concepts and measures to be viewed validly as a general perceptual style, it must also be associated with perceptual experience in other modalities.

Extending Concepts: Associations with Measures in Other Modalities

Pain tolerance and loudness magnitude estimation were used to test whether any of the above concepts and kinesthetic measures were associated with subjects' responses in other modalities. These two measures were chosen for both theoretical and empirical reasons.

The theoretical rationale for the use of pain tolerance follows Petrie's (1967) reasoning: If there is a style of amplification-attenuation of sensory experience, amplifiers should be less tolerant of pain than attenuators because they experience it as more intense. Since two hypotheses of amplification-attenuation are being examined in this study, Petrie's augmentation-reduction and amplification-attenuation as presented in this paper, pain tolerance is an appropriate measure for testing

the extension of these concepts. Empirically, pain tolerance has been shown to be related to performance on the kinesthetic task, most often to change scores after large block intervening stimulation (Petrie, 1967; Petrie et al., 1958; Ryan & Foster, 1967; Sweeney, 1966). Although Petrie (1967) interprets such evidence as supporting the augmentation-reduction construct, it actually only provides information as to change scores on the large block task, scores which may well reflect contrast effects. For only one of Petrie's clinical subjects does she report performance on both the large and small block intervening stimulation tasks (Petrie, 1967). Morgan et al. (1970) performed the only previous study using both large and small block intervening stimulation with a group of subjects, and they found no association between classification as augmenter or reducer and pain ratings. Experiment 2 was the first study to use both large and small block tasks and pain actually tolerated. This design allowed for a test of the augmentation-reduction versus contrast effects hypotheses to see which, if any, explained the previously found association between kinesthetic change scores and pain tolerance.

Concepts of amplification-attenuation define individual differences in the subjective experience of the intensity of the same, "objective," stimulus. The relationship between subjective and objective stimulus intensity is well studied in psychophysics and yields replicable power functions relating subjective magnitude estimation and actual stimulus intensity in various modalities (Lindsay & Norman, 1972; Marks &

Stevens, 1966; Stevens, 1962; Stevens & Guirao, 1964; Wanschura & Dawson, 1974). Psychophysical magnitude estimation tasks therefore provide an indicator of a person's subjective experience of the intensity of stimuli. Individual differences in psychophysical power functions are documented, with the source of these differences a subject of debate among psychophysical researchers (Ekman & Akesson, 1965; Ekman, Hosman, Lindman, Ljungberg, & Akesson, 1968; Engeland & Dawson, 1974; Jones & Marcus, 1961; Künnapas, Hallsten, & Soderberg, 1973; Markley & Rule, 1965; Rule, 1966; Rule & Markley, 1971; Stevens & Guirao, 1964; Wanschura & Dawson, 1974). The position held by Stevens is that individual differences reflect error and that there is a single "true" exponent for each modality. Few other researchers agree that the individual differences in exponents are purely error, and they hypothesize that these differences in exponents are a function of differences in response set and/or in perceptual sensitivity. Differences in perceptual sensitivity could be central and/or peripheral in origin. The concept of an amplifying-attenuating perceptual style suggests a central component generating individual differences in sensitivity. Psychophysical magnitude estimation tasks thus provide an interesting test of amplifying-attenuating hypotheses, and if such an hypothesis is confirmed it would demonstrate the role of differences in perceptual sensitivity in the individual differences in psychophysical functions.

It should be noted that magnitude estimation curves reflect relative,

proportional increases in subjective intensity rather than absolute increases. These curves thus indicate the rate of acceleration of subjective experience rather than absolute levels. Individuals with the same rate of acceleration but different absolute levels of subjective response to stimuli would show the same magnitude estimation curve with the same exponent, although theoretically, one curve would lie above and parallel to the other. Such absolute levels of subjective experience cannot be documented by the magnitude estimation method. Either or both the absolute and relative increases could be expected in an amplifying-attenuating perceptual style. In addition to the difficulty in documenting absolute subjective experience, previous research provides reason for focusing on the relative increases reflected in the magnitude estimation tasks. Average evoked cortical response (AER) research suggests that AER augmenters and reducers differ most from each other at higher brightness intensities (Buchsbaum & Pfefferbaum, 1971). Average evoked responses provide an "absolute" measure in the amplitude of the subject's evoked response. If AER augmenters' amplitudes are greater than reducers' at higher intensities but not at lower intensities, then augmenters' amplitude-intensity functions must necessarily show greater acceleration, i.e., steeper slope, than reducers'. Average evoked response amplitude-intensity functions have been shown to be related to both magnitude estimation functions (Franzén & Offenloch, 1969; Keidel & Spreng, 1965; Rosner & Goff, 1967) and to change scores on the kinesthetic task (Blacker et al., 1968;

Buchsbaum & Silverman, 1968; Silverman et al., 1969; Spilker & Callaway, 1969). The methodology and pattern of results in the AER-kinesthetic studies makes it difficult to decide whether the kinesthetic measure involved should be interpreted as Petrie's augmentation-reduction measure or as contrast effects. The two studies which directly studied the relationship between the kinesthetic task and magnitude estimation tasks found amount of change after intervening stimulation to be positively correlated with magnitude estimation exponents (Cavonius et al., 1974; Sales & Throop, 1972), even though the kinesthetic task in the Sales and Throop study tended to induce decreases while in the Cavonius et al. study it induced increases. This suggests that contrast effects are associated with magnitude estimation exponents, though the Cavonius et al. study complicates the picture because it included the use of baseline measures and deviated from the usual magnitude estimation instructions to subjects. Thus there were both conceptual and empirical reasons for testing the competing hypotheses on a magnitude estimation measure.

Including the pain and magnitude estimation measures also allows an independent test of the augmentation-reduction and contrast effects hypotheses without needing to rely solely on change scores. When the correlation between change and another variable is calculated, the partial correlation method using post-stimulation score and partialling out baseline score (Bereiter, 1963) can be used. With this method, the post-stimulation and baseline scores from which change scores are derived

are used instead of change scores in the correlation. For confirmation of Petrie's augmentation-reduction hypothesis, these other variables should correlate significantly in the same direction with signed change on both large and small block tasks. For confirmation of the contrast effects hypothesis, the other variables should correlate significantly in opposite directions with signed change on the large versus the small block task. Let us examine the pattern of associations required to extend each hypothesis to the point that it can be interpreted as a general perceptual style.

Petrie's Augmentation-Reduction

Petrie's (1967) concept and measure of augmentation-reduction requires that pain tolerance be negatively correlated with signed change scores on both the large and small block tasks. Reducers on Petrie's kinesthetic tasks should have higher pain tolerance scores than augmenters, and moderates' pain tolerance scores should lie between reducers' and augmenters'. This is what Petrie has argued, but the studies she cites to confirm this have used large block stimulation only. By including both the large and small block intervening stimulation tasks, the present study provided a more accurate and appropriate test of Petrie's hypothesis. Using the two kinesthetic tasks made it possible to classify subjects as augmenter, reducer, or moderate according to Petrie's stated criteria rather than trying to infer moderate and augmenter classifications from only large block change scores.

Although I have argued above (pp. 55-56) that augmenters could be expected to have steeper magnitude estimation functions than reducers, Petrie's (1967) interpretation of the augmentation-reduction continuum does not require this to be true. Petrie could argue that augmenter-reducer differences are absolute and similar at all intensities such that this difference would not show up on magnitude estimation tasks.

Contrast Effects

If contrast effects are associated with a perceptual style, magnitude of contrast effects should be positively associated with pain tolerance. This would mean that signed change scores on the large block task would be negatively associated with pain tolerance and signed change scores on the small block task would be positively associated with pain tolerance. This prediction is based on the results of earlier studies rather than on a theoretical rationale. Several studies have shown that the greater the large block change scores, the greater the pain tolerance (Petrie, 1967; Petrie et al., 1958; Ryan & Foster, 1967; Sweeney, 1966), but there have been no studies showing a corresponding association with small block intervening stimulation. Change scores should be similarly associated with magnitude estimation exponents; signed large block change scores should be negatively correlated with magnitude estimation exponents, and signed small block change scores should be positively correlated with the exponents. The Sales and Throop (1972) and to some extent the Cavonius et al. (1974) studies support this hypothesis.

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<u>Amplification-Attenuation Hypothesis</u>

This hypothesis is similar in concept to Petrie's (1967) but involves a different operationalization thought to more directly reflect the meaning of the concept. The results of Experiment 1 indicated that there are consistent individual differences in kinesthetic size judgments such that some people judge a block to be larger than it really is, others judge it as smaller, and some judge it to be about the size that it is. The amplification-attenuation hypothesis requires that similar amplifying-attenuating tendencies be present for any given individual in other modalities as well, such that the person who amplifies/attenuates the size of a block would also amplify/attenuate experience of pain and stimuli in other modalities. This hypothesis therefore predicts that kinesthetic size judgments would be negatively associated with pain tolerance and positively associated with magnitude estimation exponents.

Hypotheses

Three competing hypotheses were tested in addition to the possibility that none of them would hold. Each hypothesis generates a different expected pattern in the data.

The <u>amplification-attenuation hypothesis</u> requires the following data pattern to emerge:

1. Baseline judgments with dominant and nondominant hands positively correlated. This is a minimal requirement for baselines to qualify

as an amplification-attenuation measure.

2. Kinesthetic baseline measures negatively correlated with pain tolerance.

3. Kinesthetic baseline measures positively correlated with magnitude estimation exponents.

Petrie's augmentation-reduction hypothesis requires the following pattern:

1. Positive correlation between signed change scores on the kinesthetic large block and small block intervening stimulation tasks. This is a minimal requirement for Petrie's augmentation-reduction construct.

2. Negative correlation between pain tolerance and signed change scores on both the large block and small block intervening stimulation tasks, and, to a lesser extent,

3. Positive correlation between magnitude estimation exponents and signed change scores on both the large and small block intervening stimulation tasks.

The contrast effects hypothesis requires the following pattern:

1. Negative correlation between signed change scores on the large and small block tasks. This means that the magnitude of change is positively correlated on these two tasks. This is a minimal requirement for individual consistency in response to contrast effects.

2. Negative correlation between signed change on the large block task and pain tolerance, and positive correlation between signed change

on the small block task and pain tolerance. This means that magnitude of change is positively associated with pain tolerance.

3. Negative correlation between signed change on the large block task and magnitude estimation exponents and positive correlation between signed change on the small block task and pain tolerance. This means that magnitude of change is positively associated with magnitude estimation exponents.

Method

Subjects

Subjects were 22 Duke University undergraduate males ranging in age from 17 to 21 years. Subjects participated through the Psychology Department subject pool to fulfill required experimental hours. The pain tolerance test was administered only with each subject's informed consent; it was made clear they would not lose experimental credit for refusing to participate in this part of the experiment. Three of the subjects were left-handed. Fourteen subjects began each session with a 10-minute hand rest period. Eight subjects began each session with a 5-minute rest period to test whether decreasing the rest period would affect the consistency of baseline judgments. These two groups differed only in duration of initial hand rest period. The rest of the experiment was administered in the same way to both groups.

Procedure

The experiment involved two sessions at least 48 hours and up to 1 week apart. The sessions on each day were structured as follows:

Day 1

5- or 10-minute hand rest

10 baseline measures on a 1.5-inch block
 half of the subjects with dominant hand, half with nondominant hand

15-minute hand rest, which begins with

Auditory magnitude estimation task (takes about 6 minutes)

Large block task
 baseline measures on 1.5-inch standard
 rubbing a 2.5-inch block
 post-stimulation measures of standard

Pain tolerance measure

Day 2

5- or 10-minute hand rest

10 baseline measures on 1.5-inch block
 hand placement alternated for each subject

Kinesthetic width magnitude estimation

15-minute hand rest, during which subject answers a brief

Pain rating questionnaire

Small block task
 baseline measures on 2-inch standard
 rubbing a 1-inch block
 post-stimulation measures of standard

Kinesthetic size judgments. The apparatus used was the tapered block and one of the 1.5-inch blocks described in the Method section for Experiment 1 (p. 30; also see instructions in Appendix A). The task was initially explained to the subject as in Experiment 1, and after being blindfolded the subject was told to "find a spot on the tapered block which feels just as wide as the block in your other hand." Subjects made 10 such measures with no intervening stimulation. On Day 1 half of the subjects had their dominant hand on the 1.5-inch standard and their non-dominant hand on the tapered block, and half had their dominant hand on the tapered block and their nondominant hand on the standard. On Day 2 right-left block placement was reversed for each subject.

Kinesthetic contrast effects and Petrie's augmentation-reduction. The apparatus and instructions for the large block intervening stimulation task are described in the Method section for Experiment 1 (p. 30, and in Appendix A). The same procedure and apparatus were used for the small block intervening stimulation task except that for the small block task the standard was 2 inches wide and the stimulation block 1 inch wide (Petrie, 1967). For both the large and small block intervening stimulation tasks, the four baseline measures were followed by three stimulation periods of 90, 90, and 120 seconds. After each stimulation period, the subject made four size judgments as he had for the baseline measures. The mean of the 12 post-stimulation measures was the post-stimulation score. Change scores were post-stimulation scores minus the mean of

the four baseline measures. Thus, if the size judgment decreased after stimulation, the change score had a negative sign; and if the size judgment increased, the change score had a positive sign.

Magnitude estimation tasks. Both loudness and kinesthetic magnitude estimation tasks were administered, the loudness task to introduce an additional modality, and the kinesthetic task to test the bridge assumed between size judgments and magnitude estimation in the same modality. For both magnitude estimation tasks, the following instructions suggested by Stevens (1971) were given:

> You will be presented with a series of stimuli in irregular order. Your task is to tell how _____ (wide, loud) they seem by assigning numbers to them. Call the first stimulus any number that seems to you appropriate. Then assign successive numbers in such a way that they reflect your subjective impression. For example, if a stimulus seems 20 times as _____, assign a number 20 times as large as the first. If it seems one-fifth as _____, assign a number one-fifth as large, and so forth. Use fractions, whole numbers, or decimals, but make each assignment proportional to the _____ as you perceive it. (p. 428)

For both magnitude estimation tasks, the first stimulus was one in the middle region of the range. A few minor changes were made in the procedure Stevens suggested as optimal because the present study focused on individual differences. Stimuli were presented in the same irregular order to each subject. (Stevens suggested a different irregular order for each subject; he was interested in the group average.)

For the auditory magnitude estimation task, subjects were presented with seven tones (at 1000 Hz), ranging from 20-80 dB in 10 dB

steps. The stimuli were presented binaurally through earphones. The set of seven tones was presented three times, each time with the seven stimuli in a different random order. All subjects heard the 21 tones in the same order and assigned a number to the tone on each trial. Individual exponents were calculated by fitting a linear regression equation to the 21 points corresponding to dB intensity on the abscissa and log assigned number on the ordinate.

For kinesthetic magnitude estimation, subjects were presented with seven blocks of widths 0.5, 1, 1.5, 2, 2.5, 3, and 4 inches. The subject was blindfolded before the blocks were put before him, and held the blocks between thumb and middle finger of his dominant hand (Stevens & Stone, 1959). The set of seven blocks was presented three times, each time with the seven stimuli in a different random order. All subjects felt the 21 blocks in the same order and assigned a number to the block on each trial. Individual exponents were calculated by fitting a linear regression equation to the 21 points corresponding to log width on the abscissa and log assigned number on the ordinate.

Pain measure. Pain tolerance was measured by the amount of pressure the subject was willing to endure on a metal cleat placed on the flat part of the tibia about 2 inches above the ankle. This particular site on the leg was used because pilot work applying the cleat to the anterior part of the tibia, as recommended by Ryan and Foster (1967) and Ryan and Kovacic (1966), suggested that one happens upon particularly sensitive

spots fairly often. This seemed to be less true of the flat part of the tibia above the ankle. Furthermore, Klausner (Note 4) confirmed that this spot is often used clinically to test pain tolerance. Pressure on the cleat was induced by a clinical sphygmomanometer measuring 0-300mm Hg and calibrated in 2mm Hg intervals. Pressure was increased manually at about 5mm Hg per second. Measures were taken for both the right and left leg to provide a more stable pain tolerance score by basing it on two measures, and also to provide an internal test of consistency of the measure. The average of these two scores was used as the pain measure.

Because the pain tolerance task was to be administered only with the subject's informed consent and it was not known how many subjects would refuse, a short pain questionnaire was also included to try to get some measure of pain tolerance from all subjects. It was unclear how effective a measure this questionnaire was. The questionnaire asked subjects to rate (0-7) the pain involved in common household accidents and in medical and dental procedures (see Appendix B for the pain questionnaire). In fact, all subjects agreed to the pain tolerance task.

Results and Discussion

The first part of this section deals with the effectiveness of the measures used in Experiment 2 and with the evidence of variation among individuals. These are important considerations in evaluating the results

of this study as they pertain to the hypotheses being tested. In the second part of this section, the results of Experiment 2 are discussed in terms of the minimal and extended criteria for each of the hypotheses tested.

Effectiveness of Measures and Individual Variation

Because the present study involved measures used in previous studies, it was possible to assess the effectiveness of the measures in this study by comparing the data generated by each measure with the corresponding data from other studies. Furthermore, some of the measures provided their own internal estimate of error.

Magnitude estimation. In the magnitude estimation tasks, linear fits to the points obtained were quite close. Average individual standard errors were 0.05 and 0.08 for the loudness and kinesthetic magnitude estimation tasks, respectively. Mean exponents obtained were close to those obtained in other studies, with 0.58 and 1.44 for loudness and kinesthetic size, respectively, as compared with 0.6 and 1.33 in other studies (Lindsay & Norman, 1972, Appendix A; Stevens, 1962; Stevens & Stone, 1959). For the loudness magnitude estimation task, the standard deviation of the exponents was 0.42, with exponents ranging between .23 and 2.22. For the kinesthetic magnitude estimation task, the standard deviation of the exponents was 0.35, with individuals' exponents ranging from 1.07 to 2.65. For each task, the highest exponent was considerably above the range in which the rest of the subjects fell (0.23-1.21 for

loudness, 1.07-1.83 for kinesthetic size). This skewness did not affect overall results: Removing the two subjects with these high exponents barely changed the correlation of exponents with the other variables.

Kinesthetic change scores. As in Experiment 1, "change score" refers to a subject's post-stimulation average minus baseline average (see first paragraph of Results, p. 33). The large and small block kinesthetic tasks resulted in significant change scores, with an average decrease of 4.2 mm on the task with large block intervening stimulation, $t(21) = 3.46$, $p < .01$, and an average increase of 3.5 mm on the task with small block intervening stimulation, $t(21) = 2.82$, $p < .02$. Split-half reliability, according to Petrie's method (1967, pp. 11-12) was +.93 which is quite reliable and closely approximates the +.97 correlation reported by Petrie. Subjects were classified according to Petrie's criteria as augmenters, reducers, moderates, and stimulus governed. This distribution and average change scores for each subgroup in this study closely approximated results obtained by Morgan et al. (1970), who also followed Petrie's method and criteria and used a 45-minute hand rest period (see Table 5). Petrie does not report her subjects' performance in as great detail. Her frequency distributions of change scores are separated for large and small block stimulation, so that it is impossible to compare individual subjects' performance on both tasks. In summary, the large and small block intervening stimulation induced significant change; change scores were internally reliable and approximated the

Table 5

Distribution of Augmenters, Reducers, Moderates, and Stimulus Governed, with Their Average Change Scores Following Large and Small Block Stimulation

	Present study (N = 22 males)			Morgan et al. (1970) (N = 21 males)			Morgan et al. (1970) (N = 16 females)		
	N	Lch	Sch	N	Lch	Sch	N	Lch	Sch
Augmenter	3	-3.0	7.9	5	-2.7	8.4	1	-2.7	8.4
Reducer	5	-8.3	2.7	4	-9.6	2.4	2	-6.6	2.1
Moderate	11	-1.7	1.8	10	-1.5	4.8	10	-2.7	2.1
Stimulus governed	3	-7.8	6.6	2	-8.1	10.2	3	-9.3	12.9

Note. Change scores are in millimeters.

reliability obtained by Petrie (1967); and subject and change score distributions were very similar to those obtained by Morgan et al. (1970). The similarity of these results to those of both Petrie (1967) and Morgan et al. (1970) is important because it indicates that similar results are obtained whether the rest period is 15 minutes, as in this experiment, or 45 minutes, as in Morgan et al. (1970). This is an important point, since the 15-minute rest period is the only way in which the present experiment deviated from the methodology Petrie prescribed.

Change scores showed considerable variability between individuals. While average change after large block intervening stimulation was -4.2 mm (SD = 3.5), change scores ranged from +.5 to -10.2 mm. On the task with small block intervening stimulation, average change was +3.5 (SD = 2.9), with scores ranging from -0.8 to +11.8 mm.

Kinesthetic size judgments. Associations between baseline and post-stimulation scores found in Experiment 1 and in previous studies were replicated here in both the large and small block tasks. For the large block task, the baseline-post-stimulation correlation was +.65, and for the small block task it was +.83. This consistency in size judgments extends to blocks of different sizes, with baseline measures for the large block task (standard = 1.5 in.) and for the small block task (standard = 2 in.) correlated +.51 (the measures were made on different days). All these correlations are significant at the .05 level of significance.

The results of Experiment 2 confirmed the between-individual

variability in kinesthetic size judgments found in Experiment 1. Size judgments for the 38-mm block ranged from 25-56 mm, while judgments for the 51-mm block ranged from 37-58 mm. Average individual standard deviations for Day 1 and Day 2 were 2.0 and 1.6 mm, respectively, while for the group they were 6.6 and 3.3 mm, respectively.

Pain tolerance measure. The correlation between the right and left leg pain scores was +.53 ($p < .02$). This is considerably lower than the immediate test-retest reliability of +.95 (N = 55) reported by Ryan and Kovacic (1966), though they seem to have repeated their measure on the same spot. For a few of our subjects, the cleat did seem to hit on a particularly sensitive spot on one leg, and this may have reduced the correlation between the two measures. Poser (1962) reported rank order test-retest reliabilities ranging between +.75 and +.85 with a mean interval of 12 days between measures. He used a pressure cuff with numerous projections, which would minimize the effect of particularly sensitive spots. This procedure was not followed here because numerous projections under pressure give a sensation of pressure rather than of pain. Woodrow, Friedman, Siegelaub, and Collen (1975) found between-session reliabilities ranging from +.48 to +.69 for tolerance of pain induced by rods pressing the Achilles tendon from either side. The present study does not provide data for the reliability of the average pain measure based on both the right and left measures, though it would tend to be higher than the reliability of one measure alone. Clark and Bindra (1956) reported

high correlations (+.65 to +.78) between pain tolerance measures in different modalities (electrical, thermal, pressure) when the average of two measures was used.

Subjects varied in their tolerance for pain, with scores ranging from 40-300 mm Hg pressure (SD = 88 mm Hg). The score of 300 was given when subjects had not asked that the pressure be removed by the time pressure approached the maximum 300 mm Hg. The questionnaire did not correlate significantly with the pain tolerance measure ($\underline{r}$ = -.25), and was not used in analyzing the results. Since all the subjects agreed to the pain tolerance procedure, there was a measure of pain tolerance for each subject even though the questionnaire had to be discarded.

In summary, an examination of the data pertaining to the kinesthetic and magnitude estimation measures used in this study indicates that they were internally reliable and/or involved little error and/or approximated data patterns in previous studies. The pain tolerance measure was minimally reliable, though it is likely that the reliability of the average of two pain tolerance measures was higher than the reliability of one measure alone. These findings suggest that the present study was methodologically adequate, so that results pertaining to the hypotheses tested are indeed substantive. Furthermore, there was considerable variation between individuals on each of the measures, so that any existing relationship between variables would not be obscured by narrow ranges of scores on any of the measures. Thus, if there is individual consistency

across the measures and modalities tested, it should be evident in this experiment. Let us turn now to an examination of the associations between the different variables in light of the competing hypotheses tested.

Testing the Three Hypotheses: Minimal and Extended Criteria

On the whole, an individual's score on one measure was unrelated to his scores on another measure. Thus, the pattern of results did not support any of the hypothesized measures of a consistent perceptual style. Results pertaining to each hypothesis are discussed separately.

Petrie's augmentation-reduction hypothesis. First, the minimum criterion necessary to support Petrie's augmentation-reduction construct was not met: The correlation between large and small block change scores was a nonsignificant -.27, in contrast to Petrie's contention that change scores are positively correlated and reflect an augmentation-reduction factor. Chapter IV contains additional material indicating that minimal criteria for Petrie's (1967) constructs are not met.

Examining the association of the other variables with both large and small block change scores provides no support for extending the augmentation-reduction hypothesis. Table 6 summarizes the correlations of kinesthetic change scores on large and small block intervening stimulation tasks with pain tolerance and with magnitude estimation exponents for loudness and kinesthetic size. None of these correlations are significant. The partial correlation method suggested by Bereiter (1963) barely

Table 6

Correlations Based on Change Scores and Partial Correlations for Kinesthetic Change and Other Perceptual Measures

	Pain	Loudness exp.	Kinesthetic exp.
Lch	-.09	.10	-.32
Partial	-.09	-.02	-.40
Sch	.02	-.08	.13
Partial	.11	-.08	.10

changes the magnitude of the correlations (see Table 6) even though post-stimulation scores with baselines partialed out are used rather than the more problematic change scores. Thus, neither minimal criteria required for confirmation of Petrie's augmentation-reduction hypothesis nor evidence for the extension of Petrie's hypothesis were found.

Because the previously found relationship between large block change scores and pain tolerance was not confirmed by correlation, the data pertaining to this association were also analyzed by comparing subjects in the more extreme groups as has been done in previous studies. The data were analyzed by comparing change scores for subjects particularly high and low in pain tolerance and by examining pain tolerance scores for subjects classified by degree of change on the kinesthetic task.

Subjects were considered high or low in pain tolerance if their pain scores were more than one SD above or below the group mean. There were three high and five low pain tolerance subjects, and a *t* test of the difference between the subgroup means yielded no significant difference in average change after large block stimulation. In fact, subjects with low tolerance for pain had a slightly greater decrease than subjects with high tolerance for pain (-4.2 versus -3.4 mm).

When subjects were categorized according to kinesthetic change scores, reducers were more tolerant of pain than those showing less than 5.4 mm decreases (moderates and augmenters), but this difference is not statistically significant (see Table 7). Thus, there is merely a trend in

Table 7

Pain Tolerance Scores (in mm Hg) for the Different Subject Categories on Petrie's Augmentation-Reduction Continuum

Categories based on both Lch and Sch scores	N	Average pain tolerance	Categories based on Lch only
Moderates	11	142	nonreducers:
Augmenters	3	153	144 SD 91
Reducers	5	187	"reducers":
Stimulus governed	3	194	190 SD 78

the direction of results of previous studies with respect to change scores, and this trend is not replicated when subjects are categorized according to pain tolerance.

The results summarized in Table 7 are of particular interest with respect to Petrie's augmentation-reduction measure and its relation to pain studies employing only large block stimulation. As can be seen from Table 7, Petrie's augmenters and reducers as determined by both large and small block change scores differ less in pain tolerance than subjects categorized as reducers and nonreducers on the basis of only large block change scores. In other words, with increasing articulation of concepts and measures, the differences between the groups diminish. In fact, if only small block scores had been used, apparent augmenters (augmenters plus stimulus governed) would have slightly higher pain tolerance scores than nonaugmenters (reducers plus moderates). The scores would then be 174 versus 156 mm Hg for "augmenters" and nonaugmenters, respectively.

In summary, neither minimal nor extended criteria are met for supporting Petrie's augmentation-reduction concept because large and small block change scores were not positively correlated with each other and because neither large nor small block change scores were correlated with any of the other variables measured. Furthermore, in examining the pain tolerance scores for augmenters and reducers, differences between the groups diminished as classification became more accurate.

Contrast effects hypothesis. The minimum criterion necessary to indicate consistent individual differences in contrast effects was not satisfied, although the data did show a tendency in that direction. As mentioned above, the correlation between large and small block scores was a nonsignificant -.27. This result is similar to the low but significant negative correlation of -.34 (N = 42) found by Morgan et al. (1970). Although the -.27 correlation in this study cannot support the contrast effects hypothesis itself, combined with the Morgan et al. (1970) data it is compatible with the interpretation that there might be some consistency in individuals' response to contrast effects, but that such a consistency accounts for a very small part of the variance of change scores.

Even though there might be a small degree of consistency in contrast effects, contrast effects are not related to the other variables examined here. As indicated in Table 6, none of the correlations between change scores and the other variables tested were significant. Thus, there was no evidence to support extending the contrast effects hypothesis to account for perceptual behavior in other modalities.

Amplification-attenuation hypothesis. Baseline kinesthetic size judgments were suggested as an alternative measure of an amplifying-attenuating perceptual style. The possibility that size judgments might reflect lateral differences rather than an amplification-attenuation of the stimulus was controlled for by having subjects make size judgments with left-right hand placement alternated over two sessions. The results of

Experiment 2 indicated that when subjects were allowed a hand rest period of at least 10 minutes, kinesthetic size judgments were fairly consistent even when dominant-nondominant hand placement was reversed. For the 10-minute rest group, the correlation between size judgments on Day 1 and Day 2 (which involved alternating hands) was +.56 and the correlation between size judgments with dominant versus nondominant hand was +.52. These figures hover around the $r = .53$ required for a significance level of .05 with 14 subjects. Thus lateral differences do not account for the previously found consistent individual differences in kinesthetic size judgments; the correlations found here fall in the lower range of between-session reliabilities (.51-.87) found for size judgments made with the same hand placement on both days.

A small group of subjects (N = 8) was allowed only a 5-minute hand rest period before making baseline judgments. In this group, the size judgments on different days with hands alternated were not consistent. Correlations between the two sets of judgments showed a nonsignificant negative trend ($r_{12} = -.26$, $r_{dn} = -.25$). Indeed, the mean magnitude of dominant-nondominant differences for the 5-minute rest group (8.1 mm) was significantly greater than for the 10-minute group (3.4 mm), $t(20) = 2.61$, $p < .02$). The 3.4-mm difference for the 10-minute group is about the size of between-session differences found when hand placement is not altered: In Experiment 1, in which dominant-nondominant hand placement remained constant, average absolute difference between baseline measures

on Day 1 and Day 2 were 3.6 mm for the 15-minute rest group and 4.1 mm for the 5-minute rest group. Thus, the magnitude of the Day 1-Day 2 difference for the 5- and 15-minute rest groups in Experiment 1 is close to that obtained for the 10-minute rest group in Experiment 2 but not for the 5-minute rest group in Experiment 2. This suggests that the dominant-nondominant difference for the 10-minute rest group can be attributed to day-to-day variability, whereas for the 5-minute group, at least half of the dominant-nondominant difference can be attributed to lateral difference. This interpretation is justified by the distribution of single baseline judgments for individual subjects. The baseline measure for each subject is the average of 10 judgments. For the 5-minute group, for 5 out of 8 subjects, there was no overlap in the distributions of the 10 judgments for dominant and for nondominant hands, and for 2 of the 8 subjects only 1 of the 10 judgments overlapped. For the 10-minute group, only 3 of 14 subjects showed no overlap, and the other 11 overlapped on more than 2 judgments. With overlap and no-overlap as categories, the distributions for the 5- and 10-minute groups were significantly different ($X^2 = 8.1$, $\underline{p} < .005$). Although it is also the case that there was more variability in judgments on Day 1 for the 5-minute group than for the 10-minute group and than for either group on Day 2, it would be difficult to argue that greater variability alone accounts for "lateral" differences because of the strikingly little overlap in the judgments for the two hands for the 5-minute group. The greater variability on Day 1 could still have

resulted in considerable overlap between measures for the two hands (days).

In summary, kinesthetic size judgments fulfilled the minimal criterion of being consistent between sessions regardless of dominant-nondominant hand placement. However, this is the case only when subjects are allowed a 10-minute hand rest period. Therefore, in considering the extension of the amplification-attenuation hypothesis with kinesthetic baselines as its basic measure, only the data for the 14 subjects who had a 10-minute rest period will be considered.

Although kinesthetic size judgments were found to be fairly consistent in both this and earlier studies, they are not consistently related to the measures in other modalities (see Table 8). Baseline measures preceding the large and small block intervening stimulation tasks are also included because they comprise a replication of initial baseline measures. Kinesthetic baseline judgments on Day 1 and for the nondominant hand correlate significantly with pain tolerance in the direction predicted by the amplification-attenuation hypothesis, but the correlation is not repeated with the other baseline measures, so that only one of four separate baseline measures is correlated with pain tolerance. This is not sufficient proof, particularly since one significant correlation would be expected by chance alone among the number of correlations in Table 8. None of the other correlations is significant. Thus, the consistency in individuals' kinesthetic size judgments does not extend into the other

Table 8

Correlations Between the Different Kinesthetic Baseline Measures and Other Perceptual Measures

Kinesthetic baseline	Pain tolerance	Loudness exponent	Kinesthetic exponent
Day 1	-.63*	-.20	.01
Day 2	-.33	.23	.17
Dominant hand	-.44	.32	.12
Nondominant hand	-.54*	-.30	.05
Large	.05	-.20	-.01
Small	.21	.00	-.11

Note. N = 14, $r = 0.53$ required for .05 level of significance.

*$p < .05$.

modalities and measures tested here, so that there is no evidence in this study to support the construct of an amplifying-attenuating perceptual style.

Similarly, the two magnitude estimation exponents, for loudness and kinesthetic size, were not significantly related (r = +.29), nor was pain associated with either of these two measures. These results, in addition to the lack of correlation of these measures with any of the kinesthetic baselines or change scores, leave no evidence of a perceptual style which is consistent across any of the modalities or measures tested.

Methodological note. The data in Experiment 2 suggest that the measure of individual consistency in kinesthetic size judgments hovers around 0.5 when any two factors (day, size, hand) are varied. When all three vary simultaneously, the correlation decreases considerably, to the 0.3 range (see Table 9). The baseline judgment for the large block task is made on Day 1 after a 15-minute rest period on a block the same size as that used in the dominant and nondominant (which are also Day 1 and Day 2) measures. Baseline judgment for the small block task is made on Day 2 after a 15-minute rest on a block half an inch (33%) larger than the one used for the other baseline measures. Different days, blocks, and hands are all factors which can decrease the consistency of size judgments. Kinesthetic size judgments are fairly consistent as long as not too many factors are varied and as long as there is an adequate rest period.

It appears that in the 0-10 minute range, the length of rest period

Table 9

Correlations of Large Block Baseline (bL) and Small Block Baseline (bS) with Other Kinesthetic Measures

Rest group	bL and Day 1	Day 2	Dom.	Non.	bS and Day 1	Day 2	Dom.	Non.
10 min.[a]	.59	.52	.39	.69	.27	.65	.53	.34
5 min.[b]	.68	-.29	.62	.09	.56	.08	.54	.13

[a] $\underline{r}$ = 0.53 required for $\underline{p}$ = .05, N = 14.

[b] $\underline{r}$ = 0.70 required for $\underline{p}$ = .05, N = 8.

can affect the results, as evidenced in both Experiment 1, which compared 5- and 15-minute rest periods, and Experiment 2, which compared 5- and 10-minute rest periods. However, the evidence suggests that there is little difference in results between rest periods longer than 10 minutes, since baseline scores were similarly reliable whether the rest period was of 10 (Experiment 2), 15 (Experiment 1), or 45 (Morgan et al., 1970) minutes duration.

CHAPTER IV

ADDITIONAL CONSIDERATIONS

Continued work with and thought about Petrie's (1967) augmentation-reduction construct led to additional considerations not yet developed when the initial hypotheses in this study were formulated. These considerations are presented in a separate chapter both because they were not anticipated initially and, more importantly, because they constitute positive evidence against Petrie's (1967) interpretation of change scores.

The data generated in Experiment 2 and those reported in the Morgan et al. (1970) study allow a detailed analysis of the distribution of change scores and frequency of classification according to Petrie's (1967) criteria. This analysis indicates that the constructs "augmenter," "reducer," "moderate," and "stimulus governed" are not appropriate to the continua to which they refer. To see how this is so, we must examine two of Petrie's basic premises: that the stimulus-governed pattern is rare in normal individuals and that an individual's performance on the large and small block tasks are related such that an augmenter/reducer/moderate on one task will perform correspondingly on the other task. These premises are the basis for Petrie's development of the constructs

augmenter, reducer, moderate, and stimulus governed with a specific theoretical loading and conceptual status.

We will first examine Petrie's contention that the stimulus-governed pattern is rare among normal individuals. Petrie (1967) repeatedly emphasized that normal adult individuals are augmenters, reducers, and moderates and that the stimulus-governed style is abnormal. She developed a theory about certain types of abnormal behavior and brain injury, saying that the stimulus-governed person experiences his perceptual world as unstable. Thus, the rarity of the stimulus-governed style among normals is theoretically important to Petrie. However, data from the present study and from Morgan et al. (1970) indicate that the "stimulus-governed" style is not particularly rare.

Petrie (1967) stated that the 5.4-mm cutoff point for augmenters and reducers divides the perceptual modulation continuum into three equal groups of augmenters, moderates, and reducers. This leaves no room for the stimulus governed. However, if it is the case that about one-third of subjects will show change scores of more than 5.4 mm in the large block task and one-third will show more than 5.4 mm change on the small block task, by chance alone, one would expect about one-ninth of subjects to show change scores of more than 5.4 mm on <u>both</u> the large and small block intervening stimulation tasks. Only if significantly less than 11% of normal subjects show more than 5.4 mm change on both tasks does the term "stimulus governed" have the conceptual status Petrie

ascribes to it. Otherwise, it merely reflects the probability of a joint event of extreme scores. Petrie's (1967) reports of frequency of stimulus-governed subjects, while sometimes inconsistent, nevertheless show them to be fairly rare among normal subjects--either 0 or 3 out of 96. On the other hand, the results of Experiment 2 and a similar analysis performed on the data reported by Morgan et al. (1970) indicate that stimulus-governed subjects appear as often as expectable by chance (see Table 10, bottom row).

In addition to the theoretical expectation for frequency of "stimulus governed" generated by Petrie's 5.4 mm cutoff point, an empirical expectation can be projected by utilizing the number of extreme scores found among subjects given the large block and small block tasks. Both Experiment 2 and the Morgan et al. (1970) study provide data for making such a prediction. Table 10 summarizes obtained frequencies and frequencies expected by chance of subjects scoring changes of more than 5.4 mm on both large and small block intervening stimulation tasks. In both the present study and in the Morgan et al. study, obtained frequencies are about what would be expected by chance alone, if not slightly above chance. (In fact, when Morgan et al. disqualify subjects according to Petrie's (1967) recommendations, obtained frequencies rise relative to the expected frequencies.) Furthermore, the empirically projected probabilities (10%) are quite close to the theoretical probability (11%) discussed above. Thus, Experiment 2 and the data of Morgan et al.

Table 10

Expected and Obtained Frequencies of Subjects Scoring More Than 5.4 mm Change on Large (L) and Small (S) Block Intervening Stimulation Tasks

	Experiment 2	Morgan et al. (1970)--all subjects
Sch > 5.4 mm	6 of 22 = 27%	16 of 42 = 38%
Lch > 5.4 mm	8 of 22 = 36%	11 of 42 = 26%
Predicted frequency of both > 5.4 mm	$\frac{48}{484}$ = 10% of 22 = 2.2	$\frac{176}{1764}$ = 10% of 42 = 4.2
Obtained frequency of "stimulus governed"	3 of 22 = 14%	5 of 42 = 12%

suggest that the "stimulus-governed" pattern is merely the chance occurrence of a joint event of extreme scores.

One could argue that, although "stimulus governed" may not deserve the conceptual status Petrie (1967) ascribed to it, a not uncommon stimulus-governed style does not necessarily preclude augmenter, reducer, and moderate being meaningful constructs. The data do not support this argument. When the three stimulus-governed subjects in Experiment 2 are excluded, the correlation between large and small block change scores for the remaining 19 subjects is -.10, rather than the significantly positive correlation required to support the augmentation-reduction construct. Similarly, when the five stimulus-governed subjects in the Morgan et al. study are excluded, the large block-small block change score correlation is -.09. It appears, then, that excluding the stimulus governed serves to narrow the range of scores and thereby decreases the negative correlation. It is not the case that stimulus-governed subjects obscure a significant positive correlation between large and small block change scores. Furthermore, analysis of the distribution of change scores in Experiment 2 and in Morgan et al. (1970) also indicates that the constructs augmenter, reducer, and moderate, like stimulus governed, do not deserve the conceptual status Petrie ascribed to them.

Petrie's contention that performance on one of the block tasks is related to performance on the other task is not supported by an analysis

of the distribution of change scores. A relationship between performance on the two tasks is necessary to support the augmentation-reduction construct. Yet, empirically, classification of a subject on one task does not increase the probability that he will be similarly classified given the second task. Although this is to be expected given the lack of significant correlation between large and small block change scores found in Experiment 2, the analysis of change score distribution provides a more potent proof and is free of possible complications arising from the use of change scores in correlations. An examination of Petrie's (1967) classification criteria and the subject distributions evidenced in Experiment 2 and in Morgan et al. (1970) illustrates that performance on the large block task is unrelated to performance on the small block task.

According to Petrie's criteria, an augmenter/reducer is someone who shows more than 5.4-mm increase/decrease on one of the kinesthetic tasks and less than 5.4-mm decrease/increase on the other task. This basically means that to qualify as an augmenter/reducer, a person has to augment/reduce on one task, and must not do the opposite (reduce/augment) on the other task. Looking at the data in Experiment 2, 8 subjects were "reducers" and 14 were nonreducers on the large block task, and 6 were "augmenters" and 16 nonaugmenters on the small block task. Therefore, the probability of being a nonreducer on the large block task is 14/22 = 0.64, and the probability of being a nonaugmenter on the small block task is 16/22 = 0.73. Because there were no increases greater

than 5.4 mm on the large block task and no decreases greater than 5.4 mm on the small block task, these two probabilities constitute the probability of being a moderate on each of the tasks. Therefore, given these distributions, an "augmenter" on the small block task must appear as a moderate (= nonreducer) on the large block task and a "reducer" on the large block task must appear as a moderate (= nonaugmenter) on the small block task. If the Petrie (1967) constructs do more than only label the different parts of the change score continua, then the probability that an augmenter on one task is a nonreducer on the other should lie well above chance, the probability that a reducer on one task is a nonaugmenter on the other should lie well above chance, and the probability that a moderate on one task is a moderate on the other should lie well above chance.

Table 11 lists the probabilities of fitting into the various categories on the kinesthetic tasks. Probabilities are based on the data from both Experiment 2 and the Morgan et al. (1970) study. The conditional probabilities represent the probability of falling into the appropriate classification on a task given one's classification on the other task (B/A = probability of event B given event A). In other words, for the conditional probabilities, a subject's performance on one of the two tasks is already known. The nonconditional probabilities (moderate on small only, moderate on large only, augmenter on small only, reducer on large only) represent the probability of a particular classification on one task alone,

Table 11

Comparison of Probabilities of Subject Classification in Different Categories When Performance on Other Task Is Unknown and Known

	Mod on S only	Mod on S/ mod on L	Aug on S only	Aug on S/ nonred on L	Red on L only	Red on L/ nonaug on S	Mod on L only	Mod on L/ mod on S
Expt. 2	0.73	0.79	0.27	0.21	0.36	0.31	0.63	0.69
Morgan et al. (1970)	0.62	0.65	0.38	0.35	0.26	0.23	0.74	0.77

independent of a subject's classification on the other task. If performance on one task is related to performance on the other task, the conditional probabilities should be greater than the nonconditional probabilities, since knowing a person's classification on one task should help in predicting that person's classification on the other task. The probabilities listed in Table 11 show that the conditional and nonconditional probabilities are virtually the same. In other words, knowing how a subject performs on one task does not increase the probability that he will perform according to the corresponding category on the other task, thus calling into question another of Petrie's (1967) basic premises. Petrie's data presentation is not detailed enough for an analysis of this sort on her subject distributions.

Although Petrie's (1967) data are not reported in the detail necessary for an analysis like the one above, some inferences about the distribution of change scores can be made from the data she presents for 99 of the 570 subjects she mentions having tested. Examining these data sheds some light on how it led Petrie to develop an augmentation-reduction construct and highlights some ways in which Petrie's distributions differed from those obtained in the present study and by Morgan et al. (1970). In three groups totaling 99 subjects, Petrie seems to have found a greater frequency and degree of crossover, that is, increases on the large block task or decreases on the small block task, than was the case either in the present study or in Morgan et al. Of Petrie's 99 subjects, 22

increased on the large block task and 34 decreased on the small block task, and of these crossovers about half were 2 mm or more and ranged as far as 9 mm. Petrie presented the data for large and small block changes separately, so it is unclear which individuals crossed over, although the relatively high positive correlations (.60 and .77) she found between large and small block change scores for 41 of these subjects suggest that increasers on the large block task probably increased on the small. The other 58 subjects were somewhat younger, ranging in age from 14-26, and for them the large block-small block change score correlation was +.40. Both the Morgan et al. (1970) and the present study showed much less crossover, both in frequency and in extent. Morgan et al. (1970) found three increasers on the large block task and four decreasers on the small block task among 42 subjects. Five of these crossovers were 1.2 mm or less. All but one of the crossovers were Petrie moderates, two of whom crossed over on both large and small tasks, thus accounting for four of the seven crossovers. Only one was a reducer. Similarly, in the present study, three of 22 subjects increased on the large block task and one decreased on the small block task. The largest crossover was 2.2 mm; the rest were less than 1 mm. All the crossovers were Petrie moderates, with one subject crossing over by less than 1 mm on each task. In Experiment 1, two of 16 subjects crossed over on the large block task. Thus Petrie (1967) subjects averaged a crossover rate about three times as great as that in either the

Morgan et al. (1970) study or the present study. This means that more of Petrie's 99 subjects showed greater increases on the large block task and more showed greater decreases on the small block task, which helps explain why Petrie was moved to abandon a contrast effects interpretation for an augmentation-reduction interpretation of change scores. However, her interpretation is clearly not supported by Experiment 2 or by the Morgan et al. (1970) study, and details regarding the performance of several hundred of Petrie's subjects are not reported.

In summary, it appears that certain important qualitative aspects of Petrie's distributions differ from those in Morgan et al. and in the present study, much like Petrie's (1967) positive correlations between large and small block change scores differ from the correlations in the latter two studies. The analyses performed on the change scores and subject distributions in the present study and for the Morgan et al. (1970) study indicate that the categories "augmenter," "reducer," "moderate," and "stimulus governed" are inappropriately applied to two separate kinesthetic change score continua.

CHAPTER V

CONCLUSIONS

The results of this inquiry indicate no consistent perceptual style based on any of the hypotheses tested. None of the various perceptual measures used correlated significantly with each other.

The amplification-attenuation hypothesis based on kinesthetic size judgments had only the most minimal support in that individuals' kinesthetic size judgments were fairly consistent. However, kinesthetic size judgments were not consistently associated with any of the other perceptual measures used in this study. It would be of interest to see if kinesthetic size judgments are related to variables more similar to this measure, for example, judgments of line length in the visual modality or the size of drawings or of copied figures, for example, the Bender Gestalt figures. Some of these variables are of clinical interest because the size of drawings or figures is often used clinically to make inferences about personality. However, a hypothesis about the meaning of such a relationship would have to be developed; for example, does it reflect a primary perceptual style relating specifically to perceived size, or is it the expression of a personality characteristic such as constriction-

expansiveness that accounts for the relationship, etc.?

There was no support for the hypothesis that kinesthetic contrast effects are associated with other perceptual measures, because kinesthetic change scores were not significantly correlated with variables in other modalities. The data in Experiment 2 provided only weak support for a small degree of consistency in individuals' contrast effects. Degree of contrast shown on the kinesthetic task with large block intervening stimulation was not significantly associated with degree of contrast shown on a kinesthetic task with intervening small block stimulation (r = -.27). However, this correlation is similar to Morgan et al. (1970) who found a low significant negative correlation of -.34 between large and small block change scores, and it may be that the larger number of subjects in their study (42 versus 22 here) contributed to the slightly higher correlation and to its being statistically significant. This suggests that if there is some consistency in degree of contrast effects induced in an individual, it is very slight and accounts for only a small part of the variance in kinesthetic change scores. The lack of strong individual consistency in aftereffects within a modality extends the findings of previous researchers that there does not seem to be an association among aftereffects in different modalities. Although Wertheimer (1955) found a moderate correlation between visual and kinesthetic aftereffects, Spitz and Lipman (1960) and Rotman (Note 5) did not replicate this finding. Rotman's study also included auditory duration aftereffects;

he found zero order correlations between the auditory aftereffects and visual and kinesthetic aftereffects.

Experiments 1 and 2 argue against Petrie's augmentation-reduction interpretation and measure in several ways. Petrie's (1967) positive correlations between large and small block change scores were not replicated in Experiment 2, and the correlation between large block and same block change scores in Experiment 1 approached zero. The findings of the present study in this regard are similar to Morgan et al. (1970) and strongly suggest that there is no common factor in kinesthetic change scores that can be viewed as augmentation-reduction. An analysis of the frequency of stimulus governed among normal subjects indicates that, contrary to Petrie's contention, "stimulus governed" is merely the chance occurrence of the joint event of extreme scores on two tasks and deserves no separate conceptual status. Similarly, the analysis of probabilities of classification on the large and small block tasks indicates that the categories "augmenter," "reducer," and "moderate" appear to merely label portions of two separate continua of change scores and therefore have no meaningful status as constructs. Thus, there is evidence that "augmenter," "reducer," "moderate," and "stimulus governed" as Petrie uses them are not meaningful constructs. Furthermore, Petrie's augmentation-reduction measure was not related to other perceptual measures, including pain.

It would be difficult to argue that Experiment 2 failed to find

patterns and associations that are really present to support the augmentation-reduction construct. Petrie's methodology was closely followed, with the exception of the use of a 15-minute rest period rather than the 45-minute rest period Petrie suggested but the need for which she did not clearly account for. The possibility of using a shorter rest period was suggested by the recognition that the stability of baseline, post-stimulation, and change scores was unaffected by decreasing the rest period (Morgan & Hilgard, 1972, as compared with Platt et al., 1971). This rationale was confirmed by the very similar subject distributions and correlational patterns found in the present study and in the Morgan et al. (1970) study which used a 45-minute rest period and by the split-half reliability obtained in Experiment 2 for the change scores which was comparable to the split-half reliability obtained by Petrie (1967). Furthermore, few studies demonstrating correlations between kinesthetic change scores and other variables use 45-minute rest periods, including the Petrie et al. (1958) study demonstrating an association between large block change scores and heat tolerance which apparently involved no rest period at all.

In fact, Experiment 2 provided an improved test of the association of Petrie's augmentation-reduction measure with independent variables because both large and small block intervening stimulation tasks were used. When only one size block is used for intervening stimulation, "augmentation-reduction" and contrast effects cannot be distinguished.

In a more recent article, Petrie (1974) herself has emphasized the importance of using both the large and small block tasks in research on augmentation-reduction. Furthermore, in the present study both change scores and partial correlations avoiding the more problematic change scores were examined. The partial correlations did not result in any significant increase in the association between change scores and other variables so that psychometric difficulties are not an explanation for lack of positive results with respect to the association of change scores with an independent variable. Furthermore, the lack of significant positive correlation between large and small block change scores cannot be attributed to psychometric difficulties in correlating two sets of change scores, because the lack of positive association represented in the correlation obtained in this study and in Morgan et al. (1970) is substantially confirmed by the analysis of change score distributions presented in Chapter IV.

Thus, both the present study and Morgan et al. failed to replicate Petrie's results. Although Petrie mentioned having tested 570 subjects, she reported large block-small block correlations for only about 100 of these subjects. Similarly, frequency of normal stimulus-governed subjects was reported for about 100 subjects, and this report contained some inconsistency. A more detailed report by Petrie of the performance of all the subjects she tested on the large and small block tasks would be quite helpful, since she had data for an unusually large number

of subjects. It would be of particular interest to see whether the data pertaining to the several hundred subjects whose performance Petrie did not detail support Petrie's report or the results of the present study and Morgan et al.

Since the data patterns required to support any one of the hypotheses tested did not even approach confirmation, the evidence against these hypotheses is fairly strong. How does one account for previous studies indicating that kinesthetic change scores are related to other variables?

One of the most striking things about the research relating to an augmentation-reduction construct was the apparent convergence of different types of evidence in support of this construct. In light of the analyses and experiments discussed in this paper, it is easier to sort out the different pieces of evidence which seemed to support this construct. Previous evidence supporting any one of these associations is not overwhelming. Most of them have been discussed above. A few require further comment.

The previously found association between pain and change scores on the large block task is one of the most difficult to account for. The present study found no significant correlation between pain and large block change scores. When subjects were classified by change scores, a trend in the direction expected on the basis of earlier studies was found, but this trend was not significant and did not hold when subjects

were classified by pain tolerance. Of the studies of pain actually tolerated, only two were experimental studies employing more than a few subjects (Petrie et al., 1958; Ryan & Foster, 1967). Petrie mentioned a study by Poser (Note 1) which this writer could not analyze because the original source was unavailable. The clinical studies Petrie (1967) discussed involved few subjects. In the Ryan and Foster study, subjects were classified according to their participation in athletics, and the groups differed on both change scores and pain tolerance, so that the association between change scores and pain tolerance may have been mediated by athletic participation. This leaves only the Petrie et al. (1958) study, which directly tested the association between large block change scores and pain tolerance on 19 subjects. Thus, the association may be based on fewer subjects than seemed to be the case initially. Furthermore, Petrie et al. did not report a rest period before subjects were administered the large block task, even though several years later Petrie (1967) insisted on the importance of a long rest period. The results of Experiments 1 and 2 support the desirability of at least a 10-minute rest period, a requirement fulfilled in Experiment 2 but apparently not in Petrie et al. (1958).

Petrie's (1967) finding a high frequency of extreme scorers on both the large and small block tasks among delinquents and the brain injured deserves comment. Petrie may be right that this is an atypical pattern, and that this may reflect an increased and consistent susceptibility to

contrast effects. If such is the case, however, it is not atypical because normals have an augmenting, moderate, or reducing perceptual style, but because "normals" tend not to show a consistent, extreme susceptibility to contrast effects.

The average evoked cortical response research comprises a major area relating to the perceptual styles examined in this paper. Technical complications made it impossible to include AER measures at the time this study was carried out. However, a study of average evoked responses is presently in progress (Marsh, Note 6) which will include some of the subjects in Experiment 2. Average evoked cortical responses have been related to pain tolerance, magnitude estimation functions, and change scores on kinesthetic tasks. It will therefore be of interest to see whether there is evidence of AER augmentation-reduction in these subjects and, if so, whether AER patterns are associated with any of the variables in Experiment 2.

APPENDIX A

INSTRUCTIONS FOR THE KINESTHETIC TASK

The following instructions were used for the kinesthetic size judgment tasks with and without intervening stimulation. The phrases in parentheses were added when the task involved intervening stimulation. These instructions are adapted from Petrie (1967).

The following instructions were given to the subject at the beginning of the session, before the hand rest period began. For left-handed subjects, right-left hand placement was reversed so that all subjects had the same dominant-nondominant hand placement.

> We are going to explore your sense of touch in these two fingers on each hand. There are no right or wrong answers. I am interested in the way in which _you_ experience things; the only right answers are what _you_ feel. Let me explain what we're going to do. We'll place your right hand on a horizontal wooden block of a certain width, then, while you're still holding this block we'll place your left hand on a horizontal block that tapers from a small width at the bottom to a greater width at the top. I will ask you to find a place on the tapered block that feels just as wide as the block in your right hand. Because the sense of touch is influenced by what you have been doing with your hands, you must rest these two fingers of both hands before we can use the blocks. Nothing should touch them while they rest. You can cross your arms or put them in any position so long as you keep these fingers from touching each other or anything else.

At the end of the rest period, similar instructions were given:

> I told you something about the blocks we are going to use. Well, we're ready for them now. Let me tell you what we'll do. We'll place your

> right hand on a horizontal block of a certain width, and your left hand on the longer block that tapers from a narrow width at the bottom to a wider width at the top. I'll ask you to find a spot on the tapered block that feels just as wide as the block in your right hand. We'll do this a few times (and then place your right hand on another block and ask you to rub it with these same two fingers) . Because it is essential that you judge by what you feel and not by what you see, we must cover your eyes with a blindfold.

After putting the blindfold on the subject, the experimenter ascertains that the blindfold feels comfortable and that the subject cannot see through the blindfold. The experimenter then guides the subject's hands to the blocks, saying,

> Let's put the two fingers of your right hand on a block; you can feel that these finger guides slide and that the block is the same width all along. Now let's put your other two fingers on another block; you can feel again that the finger guides slide and that this block gets wider as you go up.

The subject is allowed to feel the tapered block only a few inches up from the narrow end. The experimenter then says

> Now show me a spot on the tapered block which feels just as wide as the block in your other hand. Find it as quickly and as accurately as you can. Say 'here' when you have found it and wait a moment until I say 'all right.' All right, now please return your hand to the bottom of the tapered block and show me again a place on the tapered block which feels just as wide as the block in your other hand.

When intervening stimulation is administered, the following instructions are added:

> Now you may rest the hand which is on the tapered block, but be careful not to let the tips of your index finger and thumb touch each other or anything else. I'm going to take away this right-hand block and put your fingers on a different block. When I say 'go ahead' would you please rub this block along its length, back and forth, at whatever rate you like. I want you to concentrate on the width of the block, so I'm not going to talk to you about anything else until you finish rubbing.

During the rubbing period, the experimenter says once,

> Please concentrate on the width of the block.
> Just a few more seconds to go.

When the rubbing period is over, the experimenter tells the subject to stop and says,

> Now I am going to put your right hand on another block; and I'll again put the fingers of your left hand on the tapered block. Now show me a spot on the tapered block which feels just as wide as the block in your other hand.

The sequence of judging and rubbing is repeated until the subject has made four judgments after each of the three stimulation periods.

APPENDIX B

PAIN QUESTIONNAIRE

The subject was asked how he would rate the following procedures on a scale from zero, not painful, to seven, extremely painful.

1. How would you rate the pain involved in the following dental procedures
   a. having your teeth cleaned
   b. having a cavity filled
   c. having a tooth pulled
2. How would you rate the pain of
   a. receiving an injection
   b. having blood drawn
3. How painful do you find accidents like
   a. cutting your finger with a knife
   b. a burn

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